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Manipulating procedural variables in a spatial precuing task *

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Spatial precuing tasks have yielded a consistent pattern of differential reaction time benefits. Specifically, precuing of two fingers on the same hand has been shown to result in faster discrete finger responses than precuing of two fingers on different hands. This phenomenon is called the hand advantage. Within the context of the spatial precuing task originally developed by Miller (1982), a series of four experiments investigated the influences of two procedural variables on the hand advantage: preparation instruction and presentation mode of preparation intervals. Two preparation instruction conditions were compared: implicit versus explicit instructions regarding preparation possibilities. Also, two presentation modes of preparation intervals were studied: a random condition, in which the preparation intervals varied randomly, and a blocked condition, in which the preparation intervals were grouped together in blocks of trials. Results showed that these two procedural variables, when manipulated independently, did not affect the hand advantage. However, when combined, they significantly reduced the hand advantage by half. Moreover, both procedural variables were shown to produce a precuing benefit for two homologous fingers on different hands. We concluded that, in spatial precuing tasks, procedural variables play an important role by inducing preparation strategies, which affect the pattern of reaction time benefits.

Using a spatial precuing task, Miller (1982) reported that precuing of two fingers on the same hand results in faster discrete finger responses than precuing of two fingers on different hands. This *hand advantage* phenomenon has been shown to be both robust and reliable as many different studies have provided successful replications (Cauraugh and Horrell 1989; Miller 1985; Reeve and Proctor 1984,

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1985, 1990; Proctor and Reeve 1986, 1988). Contrary to its existence proof, however, explanation of the hand advantage has led to much debate (Miller 1982, 1985; Reeve and Proctor 1984, 1985). Miller (1982, 1985) proposed that the hand advantage results from *motoric preparation* processes, while Reeve and Proctor (1984, 1985) argued that *nonmotoric decision* (stimulus–response translation) processes underlie the hand advantage (see also, Cauraugh and Horrell 1989; Hendrikx 1986). The purpose of the present study was to evaluate whether two procedural variables, i.e. preparation instruction and presentation mode of preparation intervals, would differentially affect the hand advantage.

The hand advantage

Miller (1982) discovered the hand advantage in a spatial precuing task, which was a modification of the movement precuing technique developed by Rosenbaum (1980). In essence, Miller's spatial precuing task is a choice reaction time (RT) task, with four possible keypress responses made by the index and middle fingers of both hands. The target stimulus is plus sign (+) in one of four possible locations in a horizontal array. The stimuli are mapped in a spatially compatible manner to the four sequentially arrayed response keys. A precue, consisting of a plus sign in two of the four possible stimulus locations, provides partial advance information, in that it indicates that the subsequent target stimulus will appear in one of the two cued locations. The precue therefore enables preliminary and selective preparation of two of the four possible finger responses.

Typically, the spatial precuing task employs three general preparation or cuing conditions. In the *prepared : hand* condition, the precue indicates that the response has to be made by one of the two fingers on either the left or right hand. In the *prepared : finger* condition, the precue indicates that the response has to be made by one of two homologous fingers on different hands. In the *prepared : neither* condition, nonhomologous fingers on different hands are precued, that is, the index finger of one hand and the middle finger of the other. Also an *unprepared* condition is included, in which the 'precue' consists of plus signs in all four locations. Following this kind of precue, the imperative stimulus appears in any of the four possible locations,

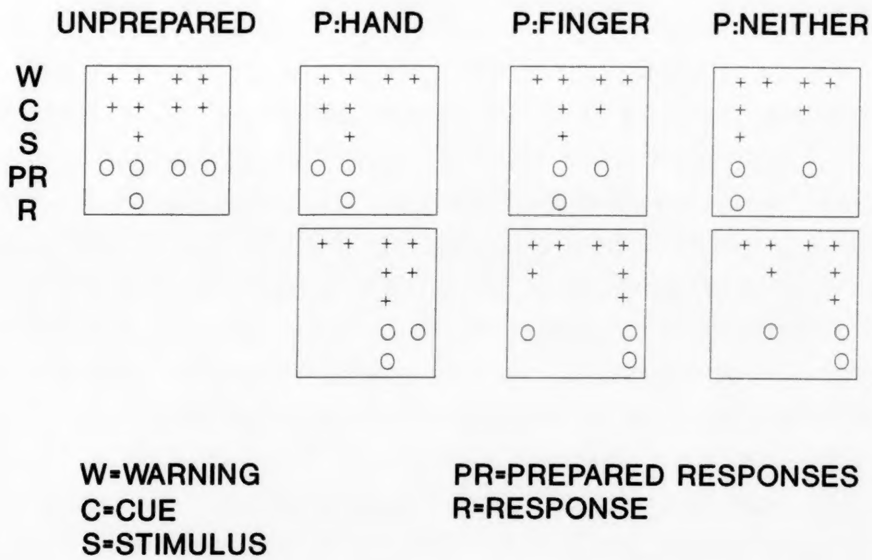

Fig. 1. A schematic representation of the spatial precuing paradigm as developed by Miller (1982).

thereby rendering selective preparation strategies of any combination of two fingers inappropriate. In fig. 1 these procedural aspects of the spatial precuing task are schematically presented.

The second independent variable manipulated in the spatial precuing task is preparation interval, that is, the time span between cue onset and target stimulus onset. In Miller's (1982) study, preparation intervals ranged from 0 ms to 1,000 ms, while subsequent studies added longer preparation intervals of up to 3,000 ms (e.g., Cauraugh and Horrell 1989; Reeve and Proctor 1984; Proctor et al. 1991).

In general, the spatial precuing task has revealed two basic results. First, the *prepared: hand* condition results in significantly faster RTs than any of the other conditions. Second, preparation of two fingers on different hands, that is, the *prepared: finger* and the *prepared: neither* conditions, do *not* result in faster RTs relative to the *unprepared* condition. This pattern of differential precuing benefits is evident primarily at short preparation intervals.

Reeve and Proctor's critique of Miller's motoric interpretation of the hand advantage

Reeve and Proctor (1984) observed that Miller's (1982) results were not in agreement with a basic result of most cuing studies: that any

type of advance information is sufficient to reduce choice RT (Goodman and Kelso 1980; Leonard 1958; Rosenbaum 1980). Indeed, it was this robust finding that had led many investigators to hypothesize a variable-order specification model of movement control (e.g., Rosenbaum 1983). Miller's finding that only the *prepared: hand* precuing condition was instrumental in reducing RT relative to the *unprepared* condition was in flagrant contrast with a variable-order control model. Instead it was in accordance with a hierarchical, or fixed order, control model of response specification, in which hand has to be specified before any other aspect of the movement. [1]

In a series of experiments Reeve and Proctor (1984; Proctor and Reeve 1986; see also Reeve and Proctor 1990) systematically examined the hand advantage reported by Miller. Their principal findings were: (1) given sufficient time (3 s) all combinations of two responses can be equally well prepared, and (2) an overlapped placements of hands (that is, fingers from each hand alternating on the response keys) showed a benefit for the *prepared: neither* precuing condition. On the basis of these findings, Reeve and Proctor postulated that precuing of finger responses is a variable process, and that the hand advantage is a 'function of the stimulus-response locations and not of whether the precued responses were on the same or different hands' (Reeve and Proctor 1984: 551). They concluded that precuing effects reflect nonmotoric 'response-selection decisions ... that occur prior to response preparation' (Reeve and Proctor 1984: 552). Subsequently, Proctor and Reeve (1986, 1988; Reeve and Proctor 1990) postulated a salient-features coding principle to characterize the operation of stimulus–response translation processes. This principle states that efficient translation is a function of the salient features of the stimulus and response sets and their correspondence.

Procedural factors in Miller's cuing procedure

The particular strength of Reeve and Proctor's hand placement manipulation (i.e., overlapped versus adjacent), which showed that the

[1] According to Miller (1982), besides an interpretation of the hand advantage as supporting a hierarchical, fixed order, control model, alternative motoric explanations are also possible, for instance, explanations based 'on a more dynamic view of the motor system (e.g., Goodman and Kelso 1980) or on the idea of differential response competition among different components of the motor system (Kornblum 1965)' (Miller 1982: 280).

precuing benefit is found for the two left-most and two right-most locations independent of the specific fingers assigned to these locations, demonstrates convincingly that the hand advantage cannot have a purely motoric origin (see for converging evidence Cauraugh and Horrell 1989; Hendrikx 1986; Stoffels 1988). However, before accepting Reeve and Proctor's interpretation of the hand advantage as reflecting stimulus–response translation processes, the possibility should be investigated whether the hand advantage is modulated by procedural factors not directly associated with processes of stimulus–response translation.

The viability of procedural factors affecting the hand advantage has been demonstrated by Reeve and Proctor (1984) in their much neglected experiment 2. Reeve and Proctor (1984) observed that when cue and target stimulus were presented simultaneously (i.e., with a preparation interval of 0 ms) the hand advantage occurred primarily because of an *interference effect* for the *prepared : finger* and *prepared : neither* conditions relative to both the *prepared : hand* condition and the *unprepared* condition. As Reeve and Proctor pointed out, this interference effect is unusual in that valid cues should be expected to produce, at worst, response latencies equivalent to that of the *unprepared* condition. In addressing this strange finding, Reeve and Proctor noted that in Miller's cuing procedure 80% of all trials consisted of nonzero preparation intervals, thereby providing on most trials some time for the deployment of preparation activities. Reeve and Proctor argued that this large percentage of trials with nonzero preparation intervals may have caused subjects 'to adopt active strategies for preparation that are, in fact, inappropriate when the target occurs at the same time as the cue' (1984: 546).

To examine the possibility that the observed interference effects reflect active preparation strategies on part of the subjects, Reeve and Proctor (1984) varied the percentage of trials on which the precue and target stimulus occurred simultaneously. They employed two conditions: one in which 20% of the trials had the 0 ms preparation interval and 80% the 3,000 ms preparation interval (the 20% simultaneous condition), and a second condition in which these percentages were reversed (the 80% simultaneous condition). Their results showed two principal findings: (1) the interference effect for the *prepared : finger* and *prepared : neither* conditions relative to the *unprepared* condition was eliminated in the 80% simultaneous condition; and (2) despite

this elimination of the interference effect in the 80% simultaneous condition, the hand advantage was still present. Reeve and Proctor (1984) interpreted the first finding as reflecting the presence of active processing strategies in Miller's cuing paradigm, and the second finding as reflecting stimulus–response translation processes. However, before accepting Reeve and Proctor's claim that the hand advantage is caused by stimulus–response translation processes, the spatial precuing paradigm should be scrutinized for other procedural variables possibly mediating the hand advantage.

Two procedural variables seem of particular interest when scrutinizing the spatial precuing task for strategic artifacts: (1) task instructions regarding preparation of responses, and (2) presentation mode of preparation interval. With respect to preparation instructions, Miller (1982) conducted two experiments in which he employed two different types of instructions to inform subjects about the functional nature of the cue. In the first experiment, subjects were informed of the fact that the target stimulus was always one of the two indicated by the precue but were not explicitly told to prepare responses indicated by the cues. In the second experiment, subjects were explicitly instructed to prepare responses indicated by the cues, but they were also informed that the target stimulus would appear in one of the expected positions on only 80% of all trials and would appear in one of the unexpected positions on the remaining trials. Both experiments showed a preparation benefit only when the cue indicated preparation of two fingers on the same hand, thereby demonstrating the hand advantage.

In trying to interpret this hand advantage phenomenon, it seems important to pay attention to the details of the stimulus–response arrangement. In both Miller's experiments the stimulus set and the response set exhibited a clear left–right distinction. In the stimulus set, the left-right distinction resulted from two blank spaces separating the inner two stimulus positions and just one blank space separating the two left-most and two right-most positions. In the response set, the left–right distinction resulted from the response fingers being placed on the two left-most and the two right-most keys on the bottom row of the key-board. This particular stimulus–response arrangement may have resulted in an advantage for the *prepared : hand* condition relative to the *prepared : finger* and the *prepared : neither* conditions, in that the cued subset of stimuli and responses in the *prepared : hand* condition constitute an obvious and natural subgroup. Therefore,

when not explicitly instructed to prepare *all possible* pairs of responses, or when instructed to do so but not all cues being valid, the saliency of the left–right distinction in both the stimulus and response sets may have led subjects to adopt active preparation strategies in the *prepared: hand* condition but not in the *prepared: finger* and the *prepared: neither* conditions. According to this line of reasoning, the hand advantage is not so much the result from subjects being *unable* or *less able* to prepare fingers on different hands, but rather from subjects not being aware of preparation possibilities in the *prepared: finger* and *prepared: neither* conditions (when not explicitly informed and instructed), or from subjects shying away from preparation of these less natural finger pairings (when not all cues are valid). In both cases, the absence of preparation benefits for the *prepared: finger* and *prepared: neither* conditions would most likely be caused by strategic factors introduced by procedural variables. Interpretation of the hand advantage in terms of efficiency of stimulus–response translation processes would, then, be inaccurate. Therefore, one aim of the present study was to investigate the effect of explicit versus implicit preparation instructions on the hand advantage.

The second procedural variable investigated in this study was presentation mode of preparation interval. In Miller's procedure, preparation interval was a within-subject variable that varied randomly within the test session, precluding subjects to anticipate the duration of the next preparation interval. Moreover, since preparation intervals were generally very short (0, 125, 250, 375, and 500 ms), subjects may have adopted the strategy of only preparing the obvious pair of responses, that is, responses indicated by the *prepared: hand* cue. A second goal of this study, therefore, was to evaluate the effects of random versus blocked presentation modes of preparation intervals on the hand advantage.

In the experiments to follow, we defined the hand advantage as the preparation benefit for the *prepared: hand* condition relative to the *prepared: finger* condition, operationalized as RT *prepared: finger* minus RT *prepared: hand*. Therefore, the *prepared: neither* condition was not included.

Overview of experiments

Using Miller's (1982) original spatial precuing paradigm, we carried out four experiments to evaluate the potential for procedural effects to operate within the

precuing paradigm and to affect the hand advantage. Two procedural variables were manipulated: (1) task instructions regarding preparation of responses (i.e., explicit versus implicit), and (2) presentation mode of preparation interval (i.e., blocked versus random). The aim of the first experiment was to replicate Miller's (1982) first experiment (the one without explicit preparation instructions) in order to establish a reliable baseline against which the effects of the manipulations of the next three experiments could be evaluated. In experiment 2 explicit preparation instructions were given to subjects. In experiment 3 preparation intervals were presented in blocks of trials. In experiment 4 these two manipulations were combined.

Experiment 1: Replicating Miller (1982)

The goal of experiment 1 was to replicate the hand advantage using Miller's (1982) cuing procedure. Successful replication was considered important in order to be confident that our experimental conditions were similar to those of Miller's. Moreover, successful replication would establish a benchmark against which the results of the next experiments could be evaluated.

Method

Subjects

Eight undergraduate students of the University of Limburg participated in this experiment. They were all right-handed and volunteered to participate.

Apparatus and stimuli

Stimuli were presented on a NEC point-plot display monitor controlled by an IBM-XT computer. Responses were made by pressing one of four keys ((Z), (X), (.), and (/)) of the keyboard (the two left-most and two right-most keys on the bottom row of the keyboard). Viewing distance was held constant at 50 cm by employing a chin rest. The computer was housed in a normally lit room, and was used to control the presentations of the stimulus displays and to record response latencies and accuracies.

Stimuli were plus (+) signs presented in the standard character set of the computer; one single plus sign was approximately 3 mm wide. The stimulus display consisted of a warning signal, a cue signal, and a target signal, with the entire display centered on the viewing monitor. The warning signal was a row of four plus signs. A blank space of 3 mm separated the two left-most and right-most positions; the two center positions were separated by a black space of 6 mm. After a delay of 500 ms, the cue signal appeared immediately below the warning signal. It consisted of plus signs in either all four positions indicated by the warning signal (*unprepared* condition) or in only two of the four possible positions (*prepared : hand* and *prepared : finger* conditions). After a variable (preparation) interval the target signal (one single plus sign) appeared immediately below the cue row, in a position always indicated by the cue. The subject's task was to respond as quickly as possible without making too many

errors to the position in which the target signal occurred by pressing the appropriate response key. Target signal and response key were mapped onto each other in a spatially compatible manner, such that a target appearing in the far left-most position was to be responded to with the left middle finger pressing the far left-most response key, etc. An intertrial interval of 1 s separated the response in a trial from the start of the next trial.

Procedure

Three preparation intervals were employed (60, 500, and 1000 ms) and five (sub-)cue conditions: *unprepared*, *prepared: hand (right)*, *prepared: hand (left)*, *prepared: finger (index)*, and *prepared: finger (middle)*. A total of 300 test trials was constructed. These consisted of 5 sets of 60 trials for each of the five (sub-)cue conditions. Each of these sets were divided equally among the three preparation intervals. These 300 test trials were *randomly* divided into three blocks of 100 trials. Therefore, both cue condition and preparation interval varied randomly within blocks of trials. A rest period of 3 min was provided between block of trials.

Subjects were given instructions regarding the nature of the task. They were told that the target stimulus would always occur in a position indicated by the cue, but they were not explicitly told to use this information to prepare responses. Thirty practice trials were given at the start of the experiment. RTs less than 150 ms or in excess of 1,500 ms were excluded from data analyses. Mean correct RTs and proportions of errors were calculated for each subject as a function of main preparation condition (*unprepared*, *prepared: hand*, *prepared: finger*) and preparation interval.

Results and Discussion

A 3×3 (preparation condition $\times$ preparation interval) within-subject analysis of variance showed a significant effect of preparation condition, $F(2,14) = 9.64$, $p < 0.01$, indicating reliably faster responses for the *prepared: hand* condition ($M = 419$ ms) than the *prepared: finger* condition ($M = 453$ ms) and the *unprepared* condition ($M = 454$ ms), which did not differ (Tukey's honestly significant difference (HSD) procedure).

The main effect of preparation interval was also significant, $F(2,14) = 46.36$, $p < 0.001$, which reflected a decrease in RT as the preparation interval increased to 500 ms ($M = 489$, 416, and 423 ms, for the preparation intervals of 60, 500, and 1000 ms, respectively). The significant interaction between preparation condition and preparation interval, $F(4,28) = 3.68$, $p < 0.05$, indicated that responses were faster in the *prepared: hand* condition than in the other two conditions but that over the first 500 ms of preparation interval this difference substantially *increased* compared to the *unprepared* condition and *decreased* compared to the *prepared: finger* condition (for preparation intervals 60, 500, and 1000 ms, mean RT for the *unprepared* condition was 486, 436, and 444 ms, respectively; for the *prepared: hand* condition 468, 394, and 396 ms, respectively; and for the *prepared: finger* condition 512, 417, and 429 ms, respectively).

Similar findings have regularly been found in other precuing studies (Cauraugh and Horrell 1989; Miller 1982, 1985; Reeve and Proctor 1984). They are also present in the other experiments of this study, but will not be discussed in detail.

Overall error rate was low: 2.08%. An analysis of variance carried out on the error data indicated only a significant difference among error rates for the three preparation conditions, $F(2,14) = 5.67$, $p < 0.025$. More errors were made in the *prepared:finger* condition ($M = 3.22\%$) than in the *prepared:hand* condition ($M = 1.14\%$) and the *unprepared* condition ($M = 1.87\%$).

In summary, by demonstrating a significant hand advantage of 36 ms, the results of experiment 1 replicated the relevant experiments of Miller (1982), Reeve and Proctor (1984), and Cauraugh and Horrell (1989). Also, baseline effects have been established; they provide a basis for evaluating the effects of the experimental manipulations of the next 3 experiments.

Experiment 2: Explicit preparation instructions

The intent of experiment 2 was to evaluate the effects of explicitly instructing subjects regarding all possible preparation possibilities on preparation benefits. Therefore, instead of using implicit preparation instructions as in experiment 1, now subjects were explicitly instructed and encouraged to engage in effortful preparation strategies in both the *prepared:hand* and *prepared:finger* conditions.

Method

Subjects

Eight undergraduate students of the University of Limburg participated in this experiment. Seven subjects were right-handed and one left-handed. They volunteered to participate in this study. None of them had performed in the previous experiment.

Procedure

Apparatus and stimuli were the same as in experiment 1. Also, the procedure of experiment 1 was followed, except for the crucial modification that subjects were *explicitly* instructed to channel effort in preparing all possible pairs of two fingers.

Results and Discussion

Mean RTs for the *unprepared* ($M = 422$ ms), *prepared:hand* ($M = 373$ ms), and *prepared:finger* ($M = 401$ ms) conditions were all significantly different from each other, $F(2,14) = 37.01$, $p < 0.001$. Therefore, as in the previous experiment, a significant hand advantage was found. This hand advantage, however, was not significantly different from the hand advantage found in experiment 1 (28 versus 36 ms, respectively; $t(14) = 0.21$, $p > 0.25$).

Importantly, a significant preparation benefit of 21 ms was demonstrated for the *prepared: finger* condition relative to the *unprepared* condition. This latter finding is in contrast with the results of experiment 1 and with the results of Miller's (1982) experiment 1, where no such preparation benefit for fingers on different hands was found. Apparently, instructing subjects about all preparation possibilities resulted in subjects adopting active preparation strategies not only in the *prepared: hand* condition but also in the *prepared: finger* condition.

Overall error rate was somewhat higher than in experiment 1: 3.98%. An analysis of variance failed to detect any significant effects.

In summary, providing subjects with explicit preparation instructions resulted in a preparation benefit for the *prepared: finger* condition relative to the *unprepared condition*. Also, a significant hand advantage of comparable magnitude to that of experiment 1 was found (28 versus 36 ms).

Experiment 3: Blocked preparation intervals

This experiment was a replication of experiment 1 with one important modification: instead of preparation intervals varying randomly from trial to trial, now the preparation intervals were grouped together in separate blocks of trials. The rationale of this manipulation was that when subjects do not know in advance how long the preparation interval will be, they may adopt the strategy of only preparing the natural finger pairings (*prepared: hand*) and not the less natural (*prepared: finger*). In other words, when preparation interval varies randomly, the very short preparation interval of 60 ms which occurs on 1/3 of all trials, may frustrate subjects' efforts to prepare, and consequently may encourage subjects to adopt the strategy of not preparing at all when confronted with the less natural *prepared: finger* pairings. On the other hand, when subjects do know in advance the duration of the preparation interval, they may adopt optimal preparation strategies in all preparation conditions.

Method

Subjects

Eight undergraduate students of the University of Limburg participated in this experiment. Seven subjects were right-handed and one left-handed. They volunteered to participate in this study. None of them had performed in the two previous experiments.

Procedure

Apparatus and stimuli were the same as in experiment 1. The procedure was similar to that of experiment 1, except for the fact that preparation interval was a blocked variable. That is, each subject received a block of 100 trials for each preparation interval, with the order of blocks being random. As in experiment 1, within a block of 100 trials, there were 20 trials for each of the five (sub-)cue conditions: *unprepared*, *prepared: hand (left)*, *prepared: hand (right)*, *prepared: finger*

(index), *prepared: finger (middle)*. The order of these cuing conditions within a block of 100 trials was random.

Results and Discussion

Mean RTs for the *unprepared*, *prepared: hand*, and *prepared: finger* conditions differed all significantly from each other (430, 364, and 394 ms, respectively), $F(2,14) = 72.56$, $p < 0.001$. In essence, this pattern of results mirrored that of experiment 2. Again, a significant hand advantage of comparable magnitude to that of experiment 1 was found (30 versus 36 ms, respectively; $t(14) = 0.34$, $p > 0.25$), and, again, a significant preparation benefit of 36 ms was demonstrated for the *prepared: finger* condition relative to the *unprepared* condition.

Fewer errors were made in the *prepared: hand* condition ($M = 1.45\%$) than in both the *unprepared* condition ($M = 4.3\%$) and the *prepared: finger* condition ($M = 5.2\%$), $F(2,14) = 8.02$, $p < 0.01$.

In summary, the effect of grouping preparation intervals together in blocks of trials was that subjects improved preparation efficiency in the *prepared: finger* condition relative to the *unprepared* condition; nevertheless, a robust hand advantage of 30 ms remained present.

Experiment 4: Explicit preparation instructions plus blocked preparation intervals

The goal of experiment 4 was to investigate the combined effects of explicitly instructing subjects regarding all preparation possibilities *and* grouping preparation intervals together in blocks of trials.

Method

Subjects

Eight undergraduate students of the University of Limburg participated in this experiment. Seven subjects were right-handed and one left-handed. They volunteered to participate in this study. None of them had performed in any of the previous experiments.

Procedure

Apparatus and stimuli were the same as in the previous experiments. Subjects were explicitly instructed to prepare all possible finger pairings (as in experiment 2), and were presented with blocked preparation intervals (as in experiment 3). Otherwise the procedure was the same as in experiment 1.

Results and Discussion

As in experiments 2 and 3, mean RTs for the *unprepared*, *prepared: hand*, and *prepared: finger* conditions were all significantly different (422, 374, and 388 ms,

respectively), $F(2,14) = 33.14$, $p < 0.001$. However, even though a significant hand advantage effect was apparent, its magnitude was significantly reduced relative to experiment 1 (14 versus 36 ms; $t(14) = 2.23$, $p < 0.025$, one-tailed). We can conclude, therefore, that the combined effects of explicit preparation instructions plus blocked preparation intervals significantly reduced the hand advantage by half.

Moreover, as in the previous two experiments, a substantial preparation benefit was demonstrated for the *prepared: finger* condition relative to the *unprepared condition* (34 ms).

Overall error rate was low: 1.62%. An analysis of variance failed to detect any significant effects, all F-values being less than 1.

In summary, the results of experiment 4 showed that the combined effects of explicit preparation instructions plus blocked preparation intervals substantially reduced the hand advantage. Also, a preparation benefit similar to that of experiments 2 and 3 was found for the *prepared: finger* condition relative to the *unprepared* condition.

General discussion

In order to scrutinize Miller's (1982) spatial precuing paradigm and the resulting hand advantage for procedural artifacts, we manipulated preparation instructions (explicit versus implicit) in experiment 2, and presentation mode of preparation interval (blocked versus random) in experiment 3. When manipulated independently, these variables did not affect the hand advantage; however, when manipulated together (experiment 4), they significantly reduced the hand advantage by half.

Therefore, we would like to argue that the hand advantage is, in part, affected by preparation strategies introduced by procedural variables. These preparation strategies presumably reflect subjects' bias to engage in effortful preparation processes only, or preferably, for natural subgroups of stimulus–response locations. Strength of subgroup is a function of the spatial layout of the stimulus–response arrangement. Since a salient left–right distinction was present in Miller's stimulus–response arrangement, subjects may have channelled more effort in preparing the natural subgroups of stimulus–response locations indicated by the *prepared: hand* cue, than the less natural subgroups of stimulus–response locations indicated by the *prepared: finger* condition. [2] In other words, when not explicitly in-

[2] Reeve and Proctor (1984) demonstrated a hand advantage using a stimulus display with *equal* distances between the four stimulus locations. One might argue that this particular kind of stimulus display does not exhibit a salient left–right distinction. However, it is well known that

structed to prepare all possible finger pairings and when provided with relative short preparation intervals (less than 1 s), subjects exclusively seem to engage in preparation processes for the *prepared: hand* condition (experiment 1; Miller 1982; Reeve and Proctor 1984). However, when made aware of the fact that also fingers on different hands can be prepared (experiment 2), and/or when the duration of the preparation interval can be anticipated (experiments 3 and 4), subjects also engage in successful preparation in the *prepared: finger* condition. This is evidenced by the fact that in experiments 2, 3, and 4, a significant preparation benefit was obtained for the *prepared: finger* condition relative to the *unprepared* condition. Therefore, preparation of two fingers on different hands *is* possible, even with short preparation intervals of 1 s and less.

This finding is important for two reasons. First, it extends the studies of Reeve and Proctor (1984, 1990) and Cauraugh and Horrell (1989), which seemed to indicate that only very long preparation intervals of 1,500 and 3,000 ms produce a preparation benefit for the *prepared: finger* condition relative to the *unprepared* condition. Second, it clearly contradicts a hierarchical, fixed order, control model of response specification, in which hand has to be specified before any other aspect of the movement (Miller 1982; Rosenbaum 1983).

Nevertheless, in all four experiments a significant hand advantage was present. What causes this hand advantage? To investigate this question at a more microscopic level, we analyzed the hand advantage as a function of preparation interval and as a function of response finger.

To investigate the effects of the experimental manipulations on the hand advantage as a function of preparation interval we conducted a 4×3 (experiment $\times$ preparation interval) analysis of variance with repeated measurements on the last factor. This analysis yielded a significant effect of experiment, $F(3,28) = 3.47$, $p < 0.05$, indicating a smaller hand advantage for experiment 4 than for the other three

human beings partition visual space into left-side and right-side parts on basis of egocentric reference axes: the body midline, the head midline, and the vertical retinal meridian (Corballis and Beale, 1983; cited in Nicoletti and Umiltà 1989). Moreover, Nicoletti and Umiltà (1989) have shown that the position of attention also leads to a powerful left–right perceptual organization. Assuming that subjects in the spatial precuing paradigm fixate the center of the stimulus display, that is, the space between the two center positions, it follows that even in Reeve and Proctor's (1984) 'equal-distance' stimulus display a potent perceptual left–right distinction may have been present.

experiments, which did not differ (Tukey's honestly significant difference (HSD) procedure). This finding confirms and strengthens the results of the between-experiment statistical t-tests. The significant main effect of preparation interval, $F(2,56) = 6.43$, $p < 0.01$, reflected a smaller hand advantage for preparation intervals 500 and 1,000 ms (18 and 19 ms, respectively) than for preparation interval 60 ms (41 ms).

Importantly, there was no significant interaction between experiment and preparation interval, $F(6,56) = 1.12$, $p > 0.2$, indicating that the smaller hand advantage in experiment 4 was achieved over the entire range of preparation intervals. This finding accords with the notion that an a priori and time-independent preparation strategy was responsible for the smaller hand advantage in experiment 4.

The finding that in all four experiments the hand advantage was largest at the very short preparation interval of 60 ms suggests that fast, perceptual cue encoding processes may contribute to the hand advantage at very short preparation intervals. This idea has recently been tested and confirmed (Adam 1991).

The hand advantage as a function of response finger is obtained by subtracting the RT data of the individual fingers in the *prepared : hand* condition from the RT data of the corresponding fingers in the *prepared : finger* condition. Fig. 2A represents the hand advantage as function of response finger averaged over experiments 1, 2, and 3. From this figure it is evident that the hand advantage is not a uniform phenomenon but that it varies as a function of response finger ($F(3,69) = 10.7$, $p < 0.001$). That is, the index fingers contribute more to the overall hand advantage than the middle fingers. Note that this finding cannot be attributed to structural differences between index and middle fingers in terms of neural, muscular, and/or skeletal factors since the *same* fingers are being compared in the *prepared : hand* and *prepared : finger* conditions.

Fig. 2B represents the hand advantage as a function of response finger found under the optimal preparation conditions of experiment 4. Again it is evident that the hand advantage varies as a function of response finger ($F(3,21) = 8.6$, $p < 0.001$). Specifically, the hand advantage in experiment 4 seems to result *exclusively* from the relative slowness of the two index fingers in the *prepared : finger* condition relative to the two index fingers in the *prepared : hand* condition. That is, the two middle fingers showed no hand advantage effect at all.

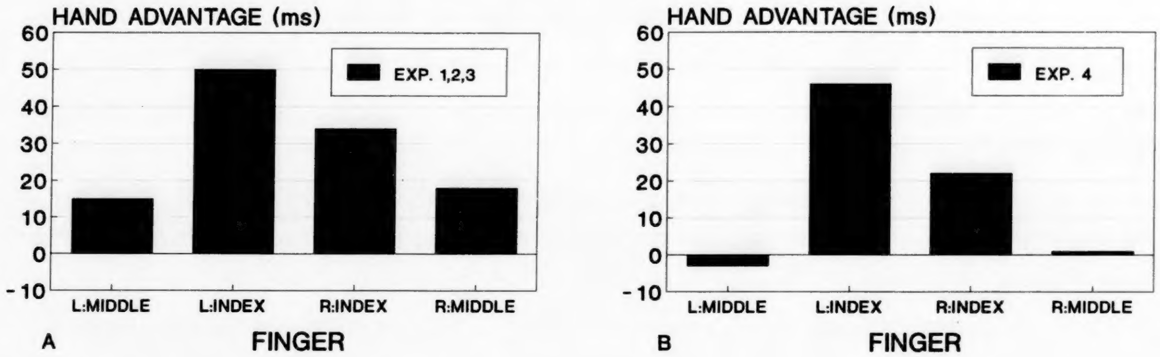

Fig. 2. (A) The hand advantage as a function of response finger averaged over experiments 1, 2, and 3. (B) The hand advantage as a function of response finger in experiment 4. (L = left; R = right.)

Clearly, this finding is at odds with a hierarchical, fixed order, control model of response specification, in which hand has to be specified before any other aspect of the movement.

In the Introduction we endorsed a stimulus–response translation account of the hand advantage as put forward by Reeve and Proctor (1984, 1990; Proctor and Reeve 1988). This account holds that the differential precuing benefits are a function of stimulus–response translation processes, or in other words, how fast the responses indicated by the cue can be determined. According to Reeve and Proctor (1984, 1985, 1990; Proctor and Reeve 1985, 1988; Proctor et al. 1990) these translation processes operate according to a salient-features coding principle. This principle states that 'the stimulus and response sets are coded in terms of the salient features of each, with response determination occurring most rapidly when the salient features of the respective sets correspond' (Reeve and Proctor 1990: 178). Reeve and Proctor (1990) postulated a hierarchy of salience for spatial locations. In particular they argued that in four-element, linear stimulus–response arrays the most salient feature is location relative to center; hence the two locations to either the left or right side of center are most salient. The two inner and two outer locations are of intermediate salience, with the alternating locations being less salient. The above results regarding the hand advantage as a function of response finger generally support the salient-features coding principle, but they also qualify it by suggesting that the two outer (stimulus–response) positions are more salient than the two inner (stimulus–response) positions. More research is needed to explain this phenomenon.

In summary, the results of this study demonstrate that, in spatial precuing tasks, procedural variables may induce differential preparation strategies, thereby possibly obscuring the processes intended to study, i.e. motor programming (Miller 1982) or stimulus–response translation (Proctor and Reeve 1988). Clearly, preparation has an important strategic component: subjects preferably engage in preparation activities when procedural constraints make it apparent, convenient and/or important to do so. Previously, other researchers have made similar observations (Requin 1980; Sanders 1983). Therefore, when one wants to create optimal, and uniform preparation conditions, we suggest that explicit preparation instructions are employed in combination with blocked preparation intervals. Failure to do so may make it difficult to unequivocally interpret a pattern of differential precuing benefits.

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The pre-choice screening of options

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Fifty years of research on behavioral decision making has produced four major revolutions in how unaided decision making is viewed. In this article we review the four revolutions, with particular emphasis on the last two – the comparative rareness of choice in real-life decision making and the two-step nature of decision making. Image Theory provides the framework for the discussion, focusing on recent empirical research on the role of screening in decision making.

Fifty years of behavioral decision research has produced four major revolutions in how unaided decision making is viewed. The early view, which still prevails in some quarters, was that all decisions were properly regarded as choices that, after extensive evaluation of the available options, resulted from maximization of expected utility (or some variation thereof).

The first revolution came from recognition that evaluation seldom is extensive and virtually never is exhaustive (e.g., Simon 1955; Tversky and Kahneman 1974).

The second revolution came from recognition that decision makers possess a variety of strategies for making choices, many of which have quite different aims than the maximization of expected utility (e.g., Beach and Mitchell 1978; Payne 1976).

The third revolution, which is still in progress, comes from recognition that choices occur relatively rarely; that past experience usually provides ways (policies, habits) of dealing with problems. Decisions are required primarily when these solutions fail, and even then the decisions may not merely be choices of the best option from some

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delineated set of options (e.g., Beach and Mitchell 1987; 1990; Klein, in press).

The fourth revolution, which is an extension and consolidation of the previous three, comes from recognition that decisions occur in steps. The first step consists of *screening out unacceptable options* and the second step consists of *choosing the best option* from among the survivors of screening (Beach 1990; Beach and Mitchell 1987; Beach and Van Zee 1992; Van Zee et al. 1992). That is, the first step focuses on what is wrong with options and the second step focuses on what is right – and the two steps are accomplished in quite different ways.

Choice, and the many strategies by which it can be accomplished, is a familiar topic in the decision literature. Screening is less familiar because it has received surprisingly little attention from decision theorists and researchers. The purpose of this paper is to describe recent work by ourselves and our colleagues on pre-choice screening of options. We will begin with a brief description of the theoretical foundations of the research, Image Theory. Then we will describe the research that has been done to date. We will close with a description of the research that currently is underway and our observations about the nature of screening and its importance as a decision phenomenon and as an area of decision research.

Foundations

Image Theory (Beach 1990; Beach and Mitchell 1987) builds upon a line of research that began with John Payne's doctoral dissertation (1976). Payne found that when presented with a set of options, decision makers tended to reduce the set using noncompensatory processes to eliminate those options that were clearly unacceptable. Then they used compensatory processes to choose an option from among the survivors of screening. In their first attempt at theory based upon these results, the Contingency Model of Strategy Selection, Beach and Mitchell (1978) proposed that decision makers have a repertory of strategies for making choices from which they select a strategy that is appropriate to the circumstances. In this model the screening process was merely the result of applying one or more of these strategies. In so far as it was recognized as separate from choice, screening was seen merely as a way of clearing away dominated

alternatives in preparation for choice, which was the main point of interest. This view of screening continues in the work that has come to be known as Contingent Processing (Payne 1982).

Beach and Mitchell (1990) found this nonexplanation of screening unsatisfactory, and, as a result, developed a broader theory of decision making. In this new view, called Image Theory, screening is not merely another strategy and the compensatory/noncompensatory distinction takes on a new meaning and importance. Image Theory sees decision making as occurring in two steps. In the first step, the set of options are screened in terms of their compatibility ('fit') with various criteria. If there is only one option to begin with and it survives, or if there are multiple options and only one survives, the sole survivor is the decision – choice is not an issue. If there are multiple options and more than one survives screening, a choice is required to break the tie, and that choice usually aims to pick the best of the survivors. In short, screening weeds out the unacceptable options and choice selects the best from among the survivors. However, choice only occurs when there is more than one survivor; a condition that is far less common than one might at first think.

Image Theory posits, and research supports, that screening is a simple, noncompensatory process. Options are considered invidually (they are not compared to one another), in terms of whether or not (1, 0) their attributes are compatible with each of the relevant criteria. If they *are* compatible, nothing happens. If they violate a criterion, we can model the violation (0, -1) as though the decision maker registered it as a -1. The option's compatibility with the criteria is merely the product of the violation and the importance weight of the criterion, summed across criteria. This produces a negative sum, where zero indicates complete compatibility with the criteria and increasingly negative sums indicate increasing incompatibility. At some point the sum is too negative (the option is too incompatible with the criteria) for the decision maker to accept options with lower sums. This point is called the *rejection threshold*, and options that fall below it are rejected.

Image Theory (Beach 1990; Beach and Mitchell 1990) fills in the details that necessarily have been omitted from the above description. However, it is necessary to note that the theory sees all options going through the screening process, even single options. If no option survives this *compatibility test*, the decision maker remains with the

status quo. If one option survives, it dictates the new course of action. If more than one option survives, any of a number of choice strategies can be used to choose the best of the survivors to determine the new course of action. Indeed, the Beach and Mitchell (1978) contingency model of strategy selection has been adapted as the part of image theory that deals with choice.

Past and current research

The first study (Beach et al. 1988) used executives of three business firms to determine if perceived acceptability of an option was a function of the number of decision criteria it violated. While the findings were complicated because it was a complicated experiment, the bottom line is that it was – the greater the number of violations the lower the perceived compatibility of the option with the firm's criteria for that decision.

A second, similar, study (Rediker 1988) using business school students as subjects and hypothetical computer firms as options for acquisition by a large business yielded similar results. In this case the results are a little easier to summarize: there was a correlation of -0.95 ($p < 0.01$) between the number of violations associated with the various options and the subjects' evaluations of the attractiveness of those options.

In the third study (Beach and Strom 1989), college undergraduates were asked to examine 14 different jobs and to decide whether they wanted to reject the job or to retain it for further consideration. The subjects were given a set of decision criteria and then proceeded to examine the attributes of each job. The attributes were written on successive pages of a small booklet (one booklet for each job) and the subject could look through the pages until he or she made a decision to reject the job or pass it on to the choice set. The jobs were designed to have different numbers of attributes that violated the criteria, and the orderings of the violations within the booklet were controlled.

The findings were very clear. Screening decisions were based exclusively upon the number of violations, consistently about 5.0, no matter how they were distributed in the sequence of information. For example, if the first 4 attributes were nonviolations, and then there were 2 violations followed by, say, 3 violations and then 3 more violations,

rejection would occur after the rejection threshold of 5 was reached. This held no matter what the distribution of nonviolations was. Indeed, nonviolations played no role in screening except to stop information search when no violations were being observed, and there was no consistent threshold for stopping search in these circumstances. Moreover, nonviolations did not compensate for violations. In short, screening was wholly accounted for by violations and there was a highly consistent rejection threshold. The relationship between compatibility (absence of violations) and survival of screening also was found in every condition in the studies to be discussed below, but in order to save space we will not belabor the point in the following descriptions.

The fourth study (van Zee et al. 1992) used both undergraduates and business students. The subjects were told to assume that they were asked by a friend who was moving to town to find a room for them to rent. The friend's criteria were stated and then preliminary information about five different rooms was given. Depending upon the condition in the experiment, some of the information about an option (room) violated the friend's criteria and some did not. The subjects then screened the rooms by designating which of them they would take the time to visit in order to find out more (survivors). Then they were given further information about the rooms that they had designated as survivors, and they choose one room for their friend.

The crucial procedure in all of this was that subjects rated the attractiveness of the rooms both after they got the preliminary information and after they got the further information about the survivors but prior to choosing a room for the friend. The surprising result was that subjects treated the two steps in the procedure as two virtually independent tasks. That is, they used the preliminary information to screen and they used the further information to choose – they did not carry forward the preliminary information for use in combination with the further information in making their choice. More to the point, the preliminary information was used noncompensatorially and the further information was used compensatorially, indicating that the two tasks were not only separate, they were done differently.

This result was very robust. It endured in the face of efforts to remind subjects to use the preliminary information (they did not) and with re-presentation of the preliminary information along with the further information (they ignored it). It was obtained for a different

task (the acquisition task used by Rediker, 1988, cited above), and re-examination of some of Payne's (1976) transcripts showed that it happened for his subjects too. In short, these results suggest that the two steps proposed by image theory are even more clearly delineated as separate tasks by decision makers.

The fifth study (Potter and Beach, in press) used the same paradigm as the Van Zee et al. study – deciding about an apartment for a friend. Here the question was what happens when after screening and assignment of survivors to the choice set it is learned that none of the survivors is still available. This is not an uncommon circumstance – one screens the applicants for a job or for graduate school admission, forms a 'short list' of acceptable candidates, and then finds that in the meanwhile all of the best candidates have accepted other offers. The same situation could exist for someone looking for a room for a friend – when it comes time to look more closely at the survivors of screening, it is found that they all have been rented.

The first experiment in the Potter and Beach (in press) study simply presented this dilemma to subjects and asked whether they would prefer to reconsider one or more of the rooms they already had rejected for their friend or whether they would prefer to start all over again with a fresh set of options. Eighty-nine percent of the subjects stated that they would prefer to start with a fresh set of options.

The second experiment required subjects to rate the importance of 13 decision criteria that are relevant to rooms for college students to live in. Then they were presented with 14 rooms to screen. After having screened the rooms the subjects were told that the rooms in their choice sets were no longer available and that no other rooms had come on the market. Because the friend had to have a room to live in, they would have to reconsider the rooms they had rejected earlier. Then they were asked to rerate the importance of the criteria and to rescreen the rejected rooms.

The results were curious but robust. Of course, when asked to rescreen rejected options, the subjects must lower their rejection thresholds or none of the options will survive to become members of the new choice set. Threshold reduction occurred, but it is not all that occurred. The subjects also reduced the rated importance of all of the criteria (in contrast to a control group that screened and rerated but that was not told that its survivors were unavailable and that it would have to rescreen). Then subjects were told that they would have to

justify their rescreening decisions – the notion being that if they were induced to think more closely about what they were doing they might be less inclined to compromise their criterion weights and might more profoundly modify their rejection thresholds instead. The justification instruction had no effect at all; the criteria were discounted just as much as in the first experiment.

Potter (1991a) found that the tendency to discount the criteria is quite robust. In a replication of the second experiment of the Potter and Beach (in press) study, one group of subjects rerated the importance of the 13 criteria immediately after rescreening (as in the original experiment). Another group rerated one hour after rescreening, and a third group rerated after 24 hours. The results for all three groups were the same as those reported in the Potter and Beach study: all three significantly discounted the importance of the criteria, $F(564, 12) = 17.97$, $p < 0.000$, and they did not differ significantly from each other.

It appears to us that discounting occurs in order to reduce subjects' discomfort when having to accept unworthy options as survivors. That is, subjects reduce the seriousness of the violations by discounting the importance of the criteria. This means that the rejection threshold, using the discounted criteria, does not have to be modified as much – that subjects can rationalize reducing their standards (the rejection threshold) by concluding that the criteria were not as important as they at first appeared.

In further research, Potter and Lituchy (1991) replicated the second experiment of the Potter and Beach study with a group of 72 Japanese business undergraduates at Nagoya University. The Japanese subjects showed no significant differences from their American counterparts. Potter (1991b) also investigated the effects of organizational role and compromise on the discounting of the importance of criteria. In a revision of the Potter and Beach study's second experiment, subjects were placed in the role of having to make the room selections as part of their job with a campus student housing office. In a 2×2 design, subjects either rated and rerated criteria importance as a work assignment (to create a rating system for the organization) or for themselves (as in the earlier experiment). The second manipulation consisted of a statement of organizational compromise in which the subject's boss claimed that the loss of desired rooms (options) was commonplace and that the subject need not worry about it. This statement was

either included or not included in the materials following the notification of the unavailability of the desired rooms.

The results revealed a significant main effect for work assignment versus doing the screening for oneself. In general, discounting was of the same magnitude as in previous studies. However, in the cell in which subjects rated the criteria for the organization but did not receive the boss's statement, discounting was 39.83% less than in the other three conditions, which did not differ significantly from each other. This suggests that discounting may be reduced when rating is part of one's job, but that it disappears when one's superior acknowledges a policy of organizational compromise in the face of option unavailability.

Clearly there is much left to learn about screening and its effects on subsequent evaluations of options for choice. In order to pursue questions related to screening, we have developed the following research agendum.

The research agendum

Table 1 shows our research agendum. It consists of eight questions – six questions that existing research has examined and the two that currently are being studied. Clearly, each question has only begun to be properly researched and much remains to be done in every case. The reason for the breadth at the price of depth is to demonstrate that Image Theory generates interesting research questions and that each question is indeed amenable to experimentation.

The first question concerns the relationship between the compatibility of options with decision criteria and judged attractiveness of the options. The results suggest that the relationship approaches being an identity.

The second question concerns the relationship between options' compatibility and their survival of screening. The results suggest that only the most compatible options survive to become members of the choice set.

The third question concerns the role of violations and nonviolations in screening. The results suggest that violations play the pivotal role and that the role of nonviolation is limited to stopping information

Table 1
The Image Theory research program on screening.

1.	Compatibility and attractiveness
	Beach et al. (1988)
	Rediker (1988)
2.	Compatibility and survival
	Beach and Strom (1989)
	Potter and Beach (in press)
	Van Zee et al. (1992)
3.	Violations and nonviolations in screening
	Beach and Strom (1989)
	Potter and Beach (in press)
	Van Zee et al. (1992)
4.	Stages in decision making
	Van Zee et al. (1992)
5.	Re-screening when survivors become unavailable
	Potter and Beach (in press)
	Potter (1991a,b)
	Potter and Lituchy (1991)
6a.	Task generalizability
	Beach et al. (1988)
	Rediker (1988)
	Van Zee et al. (1992)
6b.	Subject generalizability
	Potter and Lituchy (1991)
7.	Decision maker role and organizational constraints
	Van Zee et al. (1992)
	Potter (1991b)
8.	Partial information
	In progress
9.	Uncertainty about future availability of options
	In progress

search when violations are not discovered (although information search often is exhaustive when the option is highly compatible with decision criteria). Moreover, the number of violations required for screening out an option appears to be rather stable for a given task, implying the existence of a rejection threshold. No such stability is found for nonviolations, implying no complementary acceptance threshold.

The fourth question concerns the existence of two stages in decision making and the carrying forward of information from the first to the second stage. The results suggest that there are indeed two stages and that subjects make a very clear differentiation. Moreover, they do not appear to carry information forward, which means that their choice

may not be the option that would be chosen if they had used all that they know about the options.

The fifth question concerns the rescreening of previously rejected options when the survivors cease to be available. The results suggest that decision makers resist rescreening. When they are forced to rescreen they discount the importance of the decision criteria in such a way that they do not have to reduce their rejection thresholds too much in order to form a new choice set.

The sixth question concerns the generalizability of the results. The data are limited, but they suggest that the results generalize across both tasks and subjects. Tasks have included screening plans for introducing a new product (Beach et al. 1988), business acquisition options (Rediker 1988; Van Zee et al. 1992), jobs (Beach and Strom 1989), and rooms (Potter 1991a,b; Potter and Beach, in press; Van Zee et al. 1992). Subjects have included executives, American and Japanese business students, and American undergraduates (Potter and Lituchy 1991). Subjects also have filled different organizational roles (executives, friend) and operated under different organizational constraints (Potter 1991b). Much the same results were obtained in all conditions and combinations.

Current research

The last two questions on the agendum (table 1) are currently under examination. The first one (#8) is concerned with what happens when decision makers have only partial information about the attributes of options so that they cannot ascertain the full degree of compatibility between the option and the criteria. It is possible that they treat absent information as violations – but this would be very stringent. It may be that they treat only the most important absent information as violations – but that still is very conservative. It may be that they simply work with the information they have – which implies that they might fail to reject an option simply because they do not know enough to reject it. It may be that they require a certain amount of information relevant to the criteria before they will even make a decision. The results should be interesting.

The ninth question concerns the role of uncertainty about future availability of options on screening. This follows from the earlier

research on rescreening, but it is different. Here the question is analogous to what happens when one is screening job candidates. If a candidate is compatible with the criteria but, perhaps because of its attractiveness, may not be available or willing to take the job were you to offer it, perhaps this operates like a violation. On the other hand, perhaps this kind of uncertainty operates on a different plane than violations. That is, it may not modify the candidate's actual compatibility with the criteria, but it still reduces the candidate's standing on some scale of attractiveness. That is, uncertainty about availability may reduce the previously observed identity between compatibility and attractiveness.

No doubt the reader could suggest other potentially interesting and profitable lines of research – screening has been so little investigated that the field is wide open. Therefore, the imaginative reader is invited to join us in exploring it.

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Projective invariance and the kinetic depth effect

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Seven experiments test the assumption that, in the kinetic depth effect, observers have reliable and direct access to the equivalence of shapes in projective geometry. The assumption is implicit in 'inverse optics' approaches to visual form perception. Observers adjusted a comparison shape to match a standard shape; both standard and comparison were portrayed as in continuous rotation in space, using a graphics computer. The shapes were either plane quadrilaterals or solid prisms. The angular difference of the planes of the shapes was varied, as was the dot density of a texture in those planes. Departure from projective equivalence was measured in six studies by measuring the planar analogue of cross ratio, and in a seventh by measuring the cross ratio for points in space. Projective equivalence was not found to be perceived uniformly, except in one experiment that did not involve rotation in depth. Otherwise changes in orientation of up to 180° about a single coordinate axis had no significant effect on matches in shape, while changes in orientation about more than one coordinate axis produced significant effects. The addition of texture and a change in rotation speed did not correct departures from projective equivalence.

The constant shape of a rotating object can be discerned in the play of its changing projections or shadows: this phenomenon is known as the 'kinetic depth effect' (Wallach and O'Connell 1953). The term is best applied to flat shapes slanted in depth or to solid forms, whose full shape becomes apparent when they move in rotation. The shapes of these objects are not easily and automatically apprehended from their static projections. The kinetic depth effect has been important in the study of vision because, like other phenomena of motion such as the phi phenomenon of apparent motion, it challenges the simile that the eye is like a camera. Articles on the kinetic depth effect empha-

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size the place of motion in study of the visual perception of form. The simile of the eye as camera no longer seems apposite when diverse phenomena of motion are considered, because in those circumstances an object's shape is not revealed by its static projection – or by mere registration of an image.

Since it has served its purpose in promoting serious consideration of dioptrics (the applied optics of the eye), we may consider the simple analogy of eye and camera dispassionately. We may agree with Ryle (1954: 110), who says: 'To say that a person's seeing a tree is in principle the same sort of affair as a negative in a camera being exposed ... will not do at all. But a great deal has been found out about seeing by working on analogies like this.' Still, contemporary explanations of the kinetic depth effect maintain a traditional emphasis on dioptrics. The simile of the eye as camera has become the simile of the eyes as stereoscopic television cameras (for example Hubel 1988: 33). In that sense the simile of the eye as camera has been extended, and not abandoned. Optical principles are studied in an effort to explain visual form perception, just as one might study optical principles to build a better stereoscopic display for television.

This programme of vision research has been called 'inverse optics': these researchers 'wish to know under what conditions the visual system can enact the inverse of the general projection equation ...' (Caelli et al. 1982: 437). According to Poggio and Koch (1985: 303): 'One of the best definitions of early vision is that it is inverse optics ... While in classical optics the problem is to determine the images of physical objects, vision is confronted with the inverse problem of recovering three-dimensional shape from the light distribution in the image'. Edelman and Poggio (1989: 947) question the crude application of 'inverse optics' to the study of vision, but they do not propose that the notion be rejected. Their hypothesis is that form perception is achieved on the basis of properties that are literally present to the eye, especially those that are preserved in the propagation of light from reflective surfaces to the retina. Rock (1985: 4) says that such a process is part of the commonly held meaning of the word 'perception'.

Under this hypothesis, form perception in the kinetic depth effect is achieved by apprehending those geometric properties of the projected image that endure through the object's rotation or translation in space. These enduring properties are thought to be apparent in a

changing projection or film while the object's form is not apparent in any static projection. (Views in which sides or vertices of the object are aligned in projection are not considered, according to the *principle of general position*. Cf. Winston 1984: 46.) Gibson (1950) identifies the requisite geometric properties as projective ones: the basic properties described in projective geometry. In other words, observers have been thought to register or sense these constant projective properties as a necessary condition of form constancy in the kinetic depth effect.

Gibson explains the kinetic depth effect as an instance of shape constancy for a moving object. He asks (1965: 64): 'How can the experience of a constant world arise from the ever changing flux of sensory impressions? This is the central puzzle', and says (op.cit., p. 60): 'The central puzzle of perception, I believe, is the problem of what is called constancy'. Gibson construes the 'problem' of constancy differently than do psychologists in Helmholtz's tradition; Gibson begins by rejecting the claim implicit in the question: that the perception of form is based on a flux of sensations. Instead he borrows two notions from geometry (1965: 68): 'One is that of *transformations* and the other is that of *invariants under transformations*. These terms, though not taught us in beginning geometry, are fundamental'. As Fodor and Pylyshyn (1981: 141) point out, unless the notions of 'direct pickup' of information and 'invariant' are made specific, then 'Gibson's account of perception is empty'. But Gibson recognizes that the transformations and invariants of interest are projective transformations and projective invariants. In his terms, both projective invariants and projective transformations carry information. He comes to emphasize projective invariants as information in his ecological optics, as when he speculates (1967: 165) that 'the function of a visual system is not to register the perspectives of things, their forms or color patches in the visual field, but to register the invariants that underlie the changing perspectives'.

The last citation provides a clue how the kinetic depth effect is important to Gibson's theory of vision: in the projection of a moving object the information carried by projective invariants is made distinct from the information carried by projective variants such as are angles or distances. And in his theory shape constancy involves information carried by invariants. 'The specific hypothesis is that the invariant component in a transformation carries information about an object and that the variant component carries other information entirely, for

example, about the relation of the perceiver to the object. When an observer attends to certain invariants he perceives objects; when he attends to certain variants he has sensations' (Gibson 1965: 65). Though Gibson allows that static perspectives do afford some 'information about the object', yet object motion serves to distinguish the two sources of information best (see also Ullman 1984: 256). In other words, the continuous transformation of the image of a medium-sized physical object in rotation should enable an observer to perceive constant properties of the object, despite the effects of perspective. Gibson's last book *The ecological approach to visual perception* alludes to other uses of the term 'invariant' that are nonmathematical, as when he speaks of a child who perceives 'an invariant cat' (1979: 271). Cutting (1986: 71) replies: 'This is loose logic. If invariants are information, a cat is a cat and not an invariant.' Still, the inspiration for Gibson's wide application of the term comes directly from geometry. (Gibson (1979: 75) says: 'What we need for the formulation of ecological optics are not the traditional notions of space and time but the concepts of variance and invariance considered as reciprocal to one another.') Projective invariance remains the central example of an invariant in Gibson's ecological optics, and Gibson predicts that these invariants are perceived univocally (or as he puts it: 'directly') in the kinetic depth effect.

Those properties which are preserved under the operations of projective geometry are projective properties; they are projective invariants. They are instantiated (even if not manifest) in the propagation of light. In other words the propagation of light provides a model for projective geometry. This characteristic of light has led some psychologists to cite a projective invariant – cross ratio – in explanation of shape constancy and the kinetic depth effect. Some have claimed even that the perception of these invariant properties is a sufficient condition of shape constancy or perceived rigidity of shape (cf. Cutting (1986: 113): 'The results of four experiments suggest that perceivers use cross ratio information in making judgments about the rigid planarity of four parallel lines. These results indicate that invariants and projective geometry may be used in everyday perception.'). Yet since projective properties do not determine Euclidean form, that claim rests on an oversight or a misunderstanding engendered by hasty application of notions borrowed from geometry and optics. Another error occurs when projective invariants are taken as the sole

criteria for psychological phenomena, that is as a foundation for shape constancy and the kinetic depth effect.

It is not only Gibson who makes such essentially geometric claims: most of his opponents make similar claims. Fodor and Pylyshyn (1981: 168) write: '... if we want to find a disagreement between Gibson and the Establishment, we shall have to look to something other than the question whether the perception of distal visual layout involves inferences from proximal visual stimulations; both sides agree that it does, albeit with unequal explicitness.' Gibson is less explicit that perception might involve inferences, and what Fodor and Pylyshyn call the Establishment view is less explicit on the *form* of the proximal data. In a later article, Fodor (1988: 189) states that the data for vision constitute 'background information about certain pervasive features of the relations between distal layouts and their proximal projections'. Though he identifies these relations as *geometrical*, he does not go as far as Gibson in specifying the form of the data, or their measure. What Gibson has done – when finally he names projective invariance as information for form perception – is to forge a precise implication from a standard tradition of research in vision. In that much, Gibson's ecological optics is not far removed from the tradition that accepts Helmholtz's doctrine of unconscious inference.

Gibson (1967: 162) speaks plainly of some of his assumptions about vision, and he makes a list of them:

> 'If invariants of the energy flux at the receptors of an organism exist, and if these invariants correspond to the permanent properties of the environment, and if they are the basis of the organism's perception of the environment instead of the sensory data on which we have thought it based, then I think there is new support for realism in epistemology as well as for a new theory of perception in psychology. I may be wrong, but one way to find out is to submit this thesis to criticism.'

One aim of this paper is to show that Gibson's proposal is flawed because one of these premisses is false. I do not mean to slight either naïve realism in epistemology or the need for a new theory of perception in psychology, and I concur with Gibson's disparagement of the notion of 'sensory data'. Simply, Gibson's argument will not establish what it claims to establish, and this for a particular reason.

Consider first the clause: '*If invariants of the energy flux at the receptors exist*...'. This condition is fulfilled in vision by projective invariants, since some projective invariants are instantiated in the propagation of light. A slide image cast by a slide projector on a

slanted wall shares projective invariants with the image of the same slide as cast upon a distant flat ceiling, or from above onto a nearby table. The same invariants can be measured on the slide itself, or at a coronal cross-section of the lens of an eye. In other words the propagation of light provides a model for projective geometry, as a matter of elementary optics. Consider then the second clause: '*if these invariants correspond to the permanent properties of the environment.*' This condition is fulfilled also, as a matter of projective geometry. The projective invariant of cross ratio bears a scalar value, and this value is unchanged under the operations of projection, rotation, size scaling, and so on. The value of the appropriate scalar is unaffected by changes in dimensionality as well, under some conditions. That is to say the invariants measureable on a light-reflecting surface correspond to the invariants measureable at the eye, in that they have the same values at both places. It is the third clause '*if they are the basis of the organism's perception of the environment*' that I claim is in error. Gibson's use of the word 'basis' is no idle choice; he means the direct and unequivocal perception of projective invariance to be the foundation of shape constancy and the kinetic depth effect. Projective invariants have been supposed to be the data of visual form perception in the sense of a *given*: that which is reasoned from, and which itself does not have to be derived.

Like many rumours, the rumour that visual form perception has to do with projective invariance begins in truth, though it may not end there. Projective invariance, I hope to show, is no foundation for the kinetic depth effect; nor is it an essential part of the explanation of that effect. However, projective invariants do provide a *scale* or *standard of measure* for assessment of the kinetic depth effect (note [1]). This claim is meant to stand in marked contrast to Lappin's (1985: 73) claim that 'These invariants in the changing stimulus patterns must be what vision measures'. It is no more than a mistake in attribution to assume that since there is a measure that can be applied in studying form perception, then the visual system itself must effect such a measurement. Wittgenstein (1967: 51e) notes that: 'we can avoid ineptness or emptiness in our assertions only by presenting the model as what it is, as an object of comparison – as, so to speak, a measuring-rod; not as a preconceived idea to which [form perception] *must* correspond.' Then departures from shape constancy can be assessed by evaluating some projective invariants, from among the

absolute rational invariants of projective geometry (chief among these is cross ratio. See Weiss (1988) for other examples of invariants in projective *differential* geometry).

Niall and Macnamara (1989, 1990) demonstrate significant departures from projective accuracy in observers' estimates under a variety of conditions. We claim that observers achieve consistent and faithful reproduction of cross ratio only when they can replicate Euclidean dimensions of shape, that is, when shape constancy is shown to be complete. Observers' estimates reflect departures from projective accuracy in two distinct situations (among others). In one set of experiments (1989: esp. 68–73) many observers completed various perspective drawings of a single building, while standing in locations at which master drawings had been formed by a technique of projection. In another situation (1990: esp. 647–652) other observers completed drawings of quadrilaterals that had been photographed at an angle on a textured background. These observers made errors in projective terms both when asked to reconstruct the original drawing as seen face-on, and also when asked to copy the shape as it lay before them. The errors persisted when observers performed a matching task instead of a production task. In both situations, a number of control conditions were run to guard against the influence of experimental artifact. Still Rock (1985: 18) has said that: 'it must be the case that that solution exists in the mind in the form of knowledge of how rotation of rigid objects yields perspective transformations of the stimulus.' Yet as we will see, it is useful to test for *departures* from projective equivalence in observer's estimates of shape in the kinetic depth effect.

The experiments that follow use the analogue of cross ratio in two dimensions, and the cross ratio of points in space to test the hypothesis that unbiased perception of projective equivalence is the basis of the kinetic depth effect (i.e., the projective hypothesis). The relevance of these quantities to projective invariance is an evident point of mathematics; it is not a psychological issue. The primary aim of this article is to report that the projective hypothesis is not confirmed for flat shapes and solid forms that undergo continuous rotation in depth. Instead, systematic departures in cross ratio are found in observers' productions, and these reflect markedly inaccurate estimation of projective properties, not accurate perception or intuition of these properties as Gibson and others have predicted.

Overview of the studies

The experiments gauge the accuracy with which projective properties can be estimated for shapes in rotation; earlier studies by Niall and Macnamara (1989, 1990) employed only stationary shapes. The new experiments represent different conditions of background texture, speed of object rotation, and orientation in space. The shapes of the first experiment are nine irregular plane quadrilaterals. Each of them rotates in depth at the same rate as a plane quadrilateral whose shape can be adjusted to match. A similar experiment (experiment 1A) compares estimates made under these conditions to estimates made when the adjustable quadrilateral is fixed in orientation in the picture plane. (This is done to assess how rotation of the adjustable figure may affect observers' responses.) Differences in spatial angle between the plane quadrilaterals are introduced in experiment 2, since orientation in space has been cited as a determinant of perceived shape. Experiment 2A assesses the accuracy with which these shapes can be reproduced when rotation is restricted to the picture plane. A texture of luminous dots is added to the planes of the quadrilaterals in experiment 3, and the density and speed of rotation of this textured display is varied in a control condition (experiment 3A). Both rotation speed and density of background texture are thought to provide crucial conditions for a vivid impression of form in depth. Some demonstrations of the kinetic depth effect depend on the

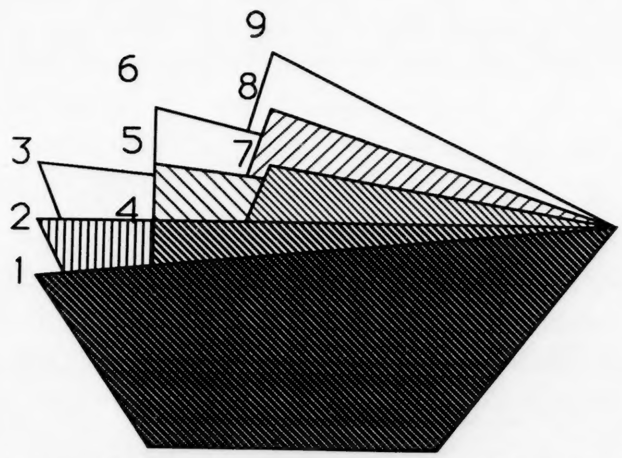


Fig. 1. The nine basic shapes of the first experiment have two sides in common. They differ in the position of a single vertex (vertex A, fig. 2). These shapes present a range of projective properties.

recognition of planar projections of solid forms; four solid forms are shown in experiment 4. The spatial angle between their bases is varied just as for the shapes of experiment 2. The experiments range over a number of geometric conditions in which the kinetic depth effect obtains.

Experiment 1: Simple quadrilaterals in tandem rotation

A plain test of the projective hypothesis is to present simple shapes in rotation. This straightforward test disambiguates Euclidean properties of shape in the picture plane, that alter with rotation in depth, from projective properties of shape that are constant in the picture plane throughout continuous rotation in depth.

Method

Subjects

Twenty students and employees of DCIEM were tested, of whom eight were men and twelve were women. Five observers had normal uncorrected acuity, and the remainder wore glasses or contact lenses for corrected acuity. The mean age of the observers was 26.7 years. None knew the purpose of the study.

Stimuli

The basic figure is a quadrilateral shape. Nine such planar quadrilaterals were used, and these differ in the position of a single vertex (see fig. 1). The various positions of the vertex are regularly spaced in terms of rectilinear coordinates.

The shapes were presented on the graphics console of an IRIS 2400 microcomputer. (The same shapes had been presented as slides in other experiments: cf. Niall and Macnamara 1990: expts. 2 and 3.) Two planar quadrilaterals were presented at once: a fixed *standard shape* and an adjustable *comparison shape*. The computer displayed the quadrilaterals in continuous rotation in depth at a rate of 20°/sec; these quadrilaterals rotated about a common axis at a single rate. The aspect ratio of the display was matched to the dimensions of its viewport, as is usually done in computer graphics displays to ensure correct perspective projection. The display consisted of (1) a three-dimensional yellow cursor centred at the origin of the x, y, and z coordinates, (2) a green comparison shape, with a three-dimensional white cursor centred on the adjustable vertex, and (3) a green standard shape, with a small white circle to mark the vertex of interest. The plane of the standard figure (the XY plane) was perpendicular to the plane of the comparison figure (either the XZ plane or the YZ plane). The z axis was the vertical axis in the initial orientation of the display; all elements of the display rotated about the z axis at the same rate. Initially the viewpoint of the observer was positioned obliquely, along the line for which $x = y = z$. The observer was free to reorient the display at any time, by making a selection on a legend (i.e., a 'menu') of successive rotations about the x, y, and z axes. In other words the

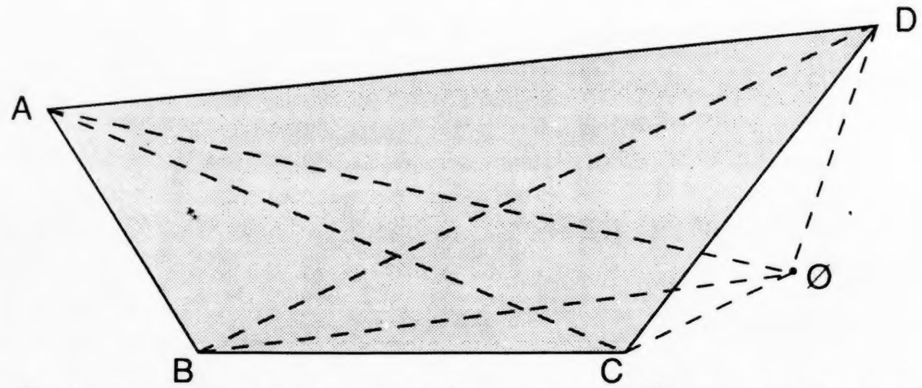


Fig. 2. The basic figure of the experiments is a quadrilateral shape. A cross ratio $(\Delta_{AD\varnothing}\Delta_{BC\varnothing})/(\Delta_{AB\varnothing}\Delta_{CD\varnothing})$ can be computed for this shape.

observer could change her orientation and angle of regard with respect to the three-dimensional coordinate axes of the display. Afterward the elements of the display continued in rotation about the reoriented z axis.

The nine standard shapes define a factor SHAPE of the experimental design. A planar analogue of cross ratio can be computed for four points in the plane and an origin of coordinates; here this measure is called 'cross ratio' for short. Denote the area of the triangle with vertices i, j, k as Δ_{ijk}. Then a cross ratio $(\Delta_{AD\varnothing}\Delta_{BC\varnothing})/(\Delta_{AB\varnothing}\ \Delta_{CD\varnothing})$ can be computed from the adjustable vertex [A] to the other vertices of the quadrilateral and the origin of coordinates (see fig. 2) [B: $(-7, -1)$, C: $(-2, -1)$, D: $(1, 3)$, $\varnothing$: $(0, 0)$]. This is set out in greater detail in Niall and Macnamara (1990: 644–645 and their fig. 6). When the computation is performed on the standard shapes, nine different values of cross ratio result. These values range from -0.45 to -1.26.

Procedure

Observers were asked to move the computer 'mouse' to alter the shape of the comparison figure. Head movements were not restrained, and observers were not under pressure of time to make their judgments. Movements of the computer 'mouse' were mirrored by changes in position of the adjustable vertex of the comparison figure, even as the comparison figure rotated. The comparison figure lay in the XZ plane on half the trials of the experiment. Then back and forth movements of the mouse changed the x coordinate of one vertex of the comparison figure, while side to side movements of the mouse changed the z coordinate of that vertex. When on half the trials the comparison figure lay in the YZ plane, then back and forth movements of the mouse changed the y coordinate of a vertex, and side to side movements changed the z coordinate. The orientation of the comparison shape (XZ plane or YZ plane) define a second factor of the experimental design, ORIENTATION. (The two ORIENTATIONs are illustrated in fig. 3). The experiment has a 9×2 repeated measures design, in which all conditions are repeated. Each observer saw the displays in a different random order. The computer programme returned the coordinates of the adjustable vertex, and these were used to calculate the cross ratio.

Results and Discussion

The dependent measure for the first analysis is the cross ratio of the comparison shape as adjusted by the observer on a single trial, minus the value of the cross ratio for the standard shape in that trial. If an observer were guided by projective properties in adjusting the comparison shape, then the dependent measure would be zero on average for all conditions of the experiment. An analysis of variance showed a significant effect of SHAPE on the dependent measure ($F(8,152) = 16.44$, $p \leqslant 0.01$). Hence cross ratios in at least one of the conditions are significantly different from the mean corrected cross ratio for the factor. The Greenhouse–Geisser correction was applied to the degrees of freedom used to obtain the critical F statistic for this comparison (Greenhouse and Geisser 1959; Grieve 1984). This conservative procedure will be applied in all significance levels reported in the present article for univariate analyses of variance. There was a smaller but still significant effect of the ORIENTATION of the comparison shapes ($F(1,19) = 7.54$, $p \leqslant 0.05$). That is, placement of the comparison shape in the XZ plane rather than the YZ plane makes some difference to the estimate. (The effect of the interaction term was not significant.) These effects can also be noticed in a simple regression between (1) the cross ratios of the standard shapes, and (2) values of the dependent variable (see fig. 4).

When the comparison shape lies in the XZ plane, the coefficient of this regression is significantly different from zero ($R(178) = 0.48$, $p \leqslant 0.001$). It is important to know

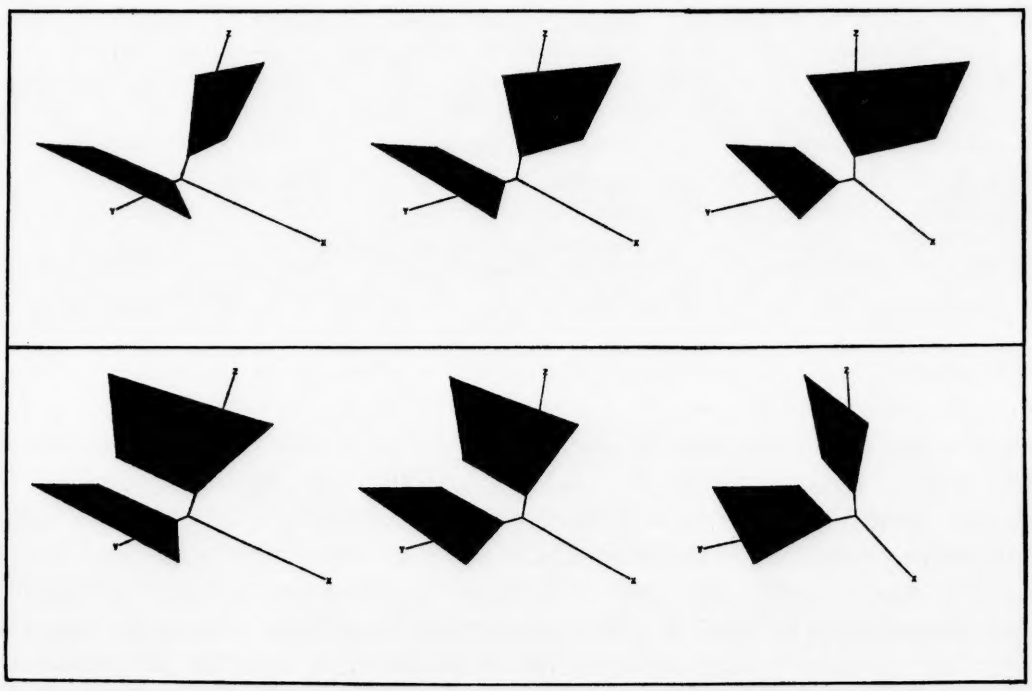


Fig. 3. The plane of the standard shape was perpendicular to the plane of the comparison shape in the first experiment. The standard shape lay in the XY plane, while the comparison shape could lie either in the YZ plane (upper half of the diagram) or the XZ plane (lower half of diagram). Three instants of rotation are depicted in either frame.

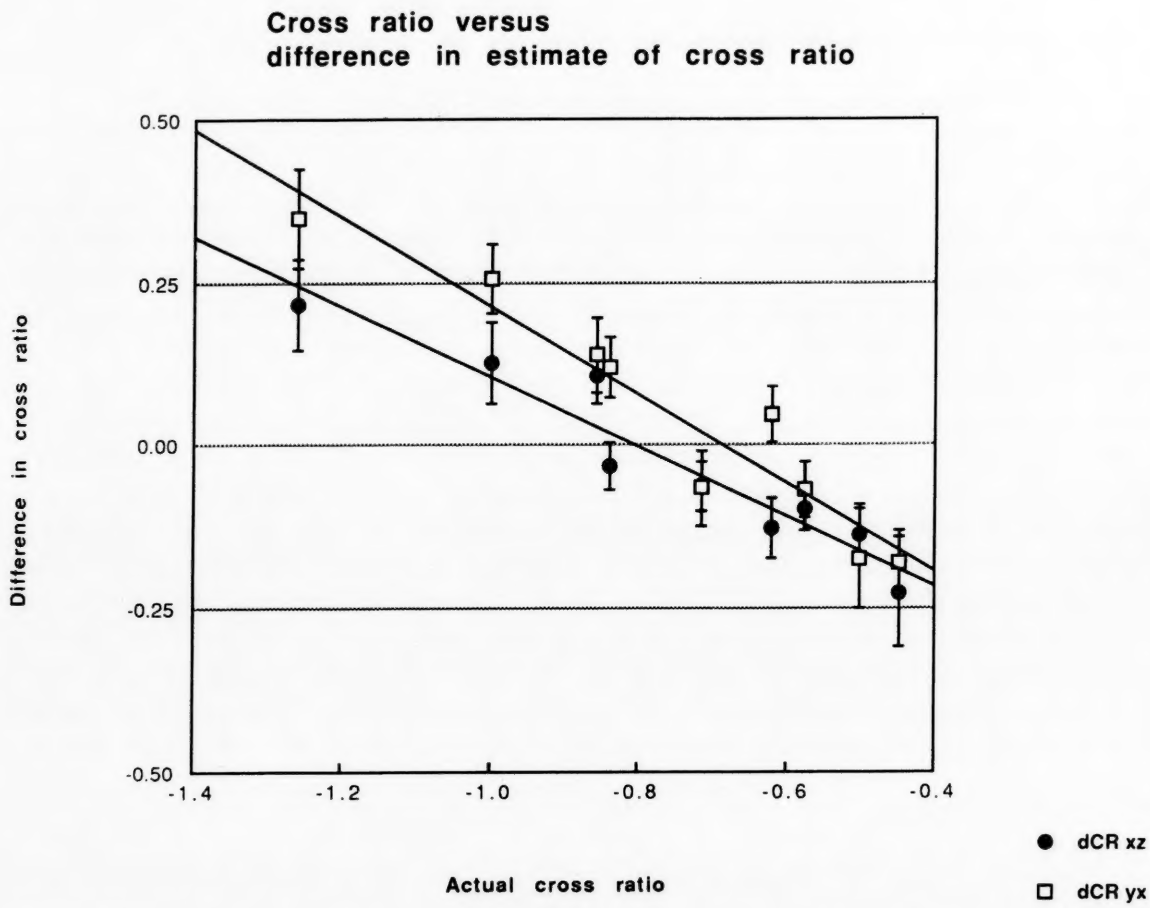


Fig. 4. Errors in estimate of cross ratio vary linearly with cross ratio in the first experiment. If observers had estimated cross ratio accurately, all points in the graph would fall along a horizontal line through zero. Twenty observers made one estimate in each condition; standard error bars are indicated. Filled circles indicate that the comparison shape lay in the *XZ* plane; unfilled squares indicate that the comparison shape lay in the *YZ* plane. Regression lines have been drawn for these two conditions of ORIENTATION.

if this effect represents departures of the cross ratio from its 'true' value. It does, as shown by the non-zero slope of the regression line (-0.53 ± 0.19, 99% confidence interval). Similarly when the comparison shape lies in the *YZ* plane, the regression coefficient is significant ($R(178) = 0.55$, $p \leqslant 0.001$) and the regression line has a non-zero slope (-0.68 ± 0.20, 99% confidence interval). These effects represent distortions in projective properties. The magnitude and direction of the errors which observers made in adjusting the white cursor is illustrated in fig. 5. Though these departures seem to be regular, it is premature to account for them in terms of Gestalt properties such as the tendency to goodness of figure, or in terms of an hypothetical distorting influence. The reader may note that the orientation of these shapes in depth is not apparent in static presentation, but becomes apparent in their rotation.

These significant effects reinforce Niall and Macnamara's (1989, 1990) claim that observers are not uniformly and reliably sensitive to projective invariance, under a

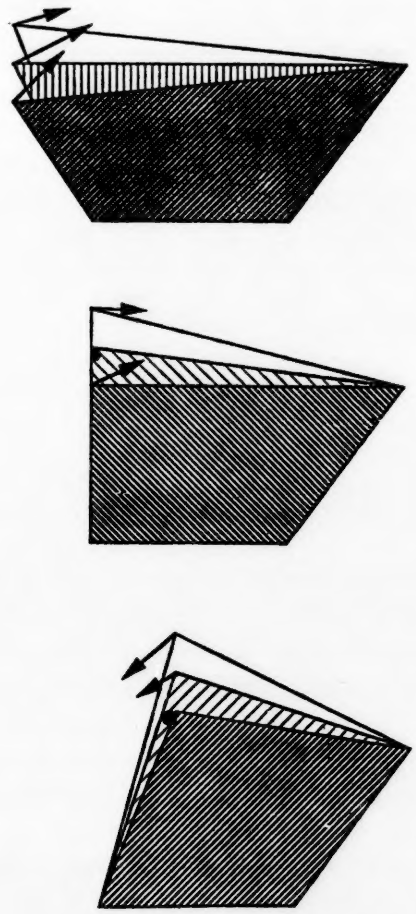


Fig. 5. Observers adjust the comparison shapes to be different on average from the standard shapes. The standard shapes are outlined or shaded; each arrowhead points to the average position of the adjustable vertex of a comparison shape.

host of conditions. Here the cross ratios of the adjusted comparison shapes are different from the cross ratios of standard shapes for this display, in which standard and comparison shapes undergo continuous rotation in depth.

Experiment 1A

There are many reasons why a production task can be difficult; not all of them are relevant to questions of perception. For example, observers may simply have been unused to making lateral and distal movements in order to adjust a shape as it rotates. It is important to establish that the results of the first experiment are not due to this rotation of the comparison shape. If the comparison shape is fixed in the picture plane, then movements of the computer 'mouse' are yoked in both speed and direction to movements of the variable vertex of the comparison shape. The distal and lateral movements of the computer mouse are linked to vertical and horizontal

movements of one vertex of the comparison shape. This comparison shape lies flush to the screen, unlike the situation in experiment 1, where the comparison shape rotates continuously in depth. That is, if the comparison shape is seen face-on and does not itself rotate, will observers be able to perform the experimental task with greater accuracy? Even if such a change makes the task easier, observers' performance may exhibit departures from accurate estimation of projective properties as before. Do observers reproduce the projective properties of shape under these conditions?

Subjects

Three adults from DCIEM were tested. They are: observer BF (22 years of age, male); observer RK (30 years of age, male, corrected to normal vision); and observer EM (19 years of age, male).

Stimuli

The nine standard shapes of the main experiment were presented in rotation as before (these form the SHAPE factor of the analysis). The comparison shapes were displayed either in the picture plane (Picture condition of factor called ORIENTATION), or in the *XZ* plane (Depth condition of factor called ORIENTATION). The comparison shapes in the picture plane did not rotate with respect to the observer; the comparison shapes in the *XZ* plane rotated about the *z* coordinate axis at a rate of 20°/sec. When the comparison shapes were displayed in the picture plane, the standard shapes were placed 40° in spatial angle from the *XY* plane to prevent direct comparison of the shapes in the picture plane at any time. The other features of the display were unchanged from experiment 1.

Method

The eighteen combinations of standard and comparison shapes were shown in different random order to the three observers. Each observer saw every display twenty times, that is, there were twenty blocks of trials for each. Here the unit of observation is taken to be a single response by an observer. Then the experiment has a three-way ($9 \times 2 \times 3$, that is, SHAPE × ORIENTATION × OBSERVER) repeated measures design. Repeated responses (in twenty trials) by an observer take the place of the responses of many observers for each condition. The procedure and instructions to observers were unchanged from experiment 1.

Results and Discussion

The unit of observation in this experiment is the single response, rather than the single individual. A feature of the observers' estimates is worthy of note. When the comparison figure lay in the picture plane (Picture condition of ORIENTATION factor), then each observer made some reversals between left-handed and right-handed (i.e., mirror-reversed) copies of the standard figure on different trials. When the comparison figure rotated in depth along with the standard figure (Depth condition of the ORIENTATION factor), no observer's estimates showed any variation in handed-

ness. Observer RK made 20 reversals (of 180 trials in the Picture condition), while observer BF made 38, and observer EM made 48. All of RK's reversal's occurred with SHAPE number *9* (see fig. 1), and every instance of this shape was reversed in his constructions. Estimates were counted as reversals when the location of the variable vertex of the comparison shape lay nearer in distance to a mirror image of its expected location than it did to its expected location in an unmirrored version of the standard figure. The mirror image of the correct point can be thought of as the final position of the correct point after the entire shape is rotated 180° in depth about the line that bisects the angle made by the two fixed sides of the quadrilateral. (The fixed sides of the standard figure are adjacent and equal in length.) The handedness of the majority of comparison figures in the Picture condition was the same for all three observers. The dependent measure was calculated in a way that is unaffected by these changes in observers' responses, taking handedness of shape into account. (Namely, the point of origin and the order of labels of points was taken to be different for 'left-handed' and 'right-handed' comparison figures. This procedure reduced the variability of the dependent measure greatly in the Picture condition.) The probability of a reversal depends on the SHAPE that is presented. That is, the probability of a left- or right-handed construction – coded as 1 or 0 – depends on the cross ratio of the standard SHAPE, as shown by point biserial correlations computed for observers BF and EM (BF: $R_{pbi} = -0.60$, $t_{178} = -10.17$; EM: $R_{pbi} = -0.37$, $t_{178} = -5.40$; both $p \leqslant 0.001$).

Analysis of variance showed a significant effect of change in ORIENTATION of the comparison shape on the dependent measure of departure in cross ratio from the correct value ($F(1,19) = 171.83$, $p \leqslant 0.01$). A significant effect of change in SHAPE was found overall ($p \leqslant 0.05$) as in previous analyses, and there was a significant difference between OBSERVERS ($p \leqslant 0.01$). All interaction effects were significant also, with the exception of the three-way interaction term.

Mean departures in cross ratio are different from zero for both ORIENTATIONs of the comparison shape ($p \leqslant 0.001$ for both conditions in all three observers), as shown by one-group *t*-tests. The signs of these departures in cross ratio are opposite for the Depth and Picture conditions. For one observer (RK), the magnitude of the departures from zero was greater in the Picture condition than in the Depth condition. Mean departures in X and Y coordinates were also significantly different from zero for the entire experiment, as shown by Hotelling's T^2 test ($T^2(2, 1078) = 125.41$, $p \leqslant 0.001$).

There are then two reasons that placement of the comparison figure in the picture plane does not improve estimates of shape. The first is that with the comparison figure placed in the picture plane, observers make left–right reversals in their consecutive judgments of a flat shape in rotation. (It may be worth noting that because left–right handedness is not a projective property – meaning a property unchanged under any projective transformation – Caelli (1979: 32) claims that the visual system cannot detect handedness in the kinetic depth effect.) The second is that despite these reversals, observers make no better estimates of shape when the comparison figure lies in the picture plane than when the comparison figure rotates in depth with the standard figure. The estimates are no better in that the value of the departures from correct cross ratio are no smaller. (RK's departures in estimate were

significantly greater in the Picture condition than in the Depth condition of the ORIENTATION factor.) Hence it seems less likely that a telling artifact could arise in the results of the first experiment as the result of a lack of correspondence in direction between the motions of the computer mouse and the yoked motions of the variable vertex of the comparison shape. The emphasis of the first experiment is to apply a stimulus measure in a new way, and as Brown and Owen (1967: 243) emphasize: 'Of the three classes of things with which psychologists have been most concerned, stimuli, individuals, and responses, lack of systematic knowledge of stimuli has perhaps been the most damaging to the programmatic study of perceptual processes.'

Experiment 2: Changes in relative orientation

When asked to compare the outlines of two flat objects, observers tend to orient the objects in certain ways. Most often they will place the objects side by side in a frontal plane for ease of comparison, as if comparison by eye is easiest for shapes that are translated left or right in the picture plane, but not rotated in any way. Indeed, sometimes it is supposed that observers must perform such an operation in imagination before making physical comparisons when objects are oriented in other ways. Shepard (1987: 91) says that this ease of comparison is accounted for by a mental representation that 'captures something of the three-dimensional structure of the object as it exists in isotropic three-dimensional space, and is relatively unrelated to its two-dimensional retinal projection ... the representation is not, however, of the inherent three-dimensional shape of the object considered purely in itself; it is always of that object as viewed in a particular orientation'.

The first experiment showed that different orientations of the standard and comparison shapes give rise to differences in estimates of shape. Since the shapes are yoked in continuous rotation, this does not reflect the features of a particular perspective view. Shepard claims that it is absolute differences in spatial orientation that are important to the comparison of shapes. Then will observers' estimates be less accurate in projective terms as difference in spatial orientation increases, or does spatial orientation affect only the effort required for the task, without affecting the accuracy of the result?

Method

Subjects

The twenty observers were the same ones who participated in the first experiment.

Stimuli

Four quadrilaterals were used in the second experiment. These shapes are marked *1*, *3*, *7*, and *9* in fig. 6; they span the range of the figures that were used in the previous experiment. The rotating shapes were displayed on the graphics console as before. The cursors and legend of the display were unchanged. The observer's viewpoint was specified to be more distant from the origin (approximately one and

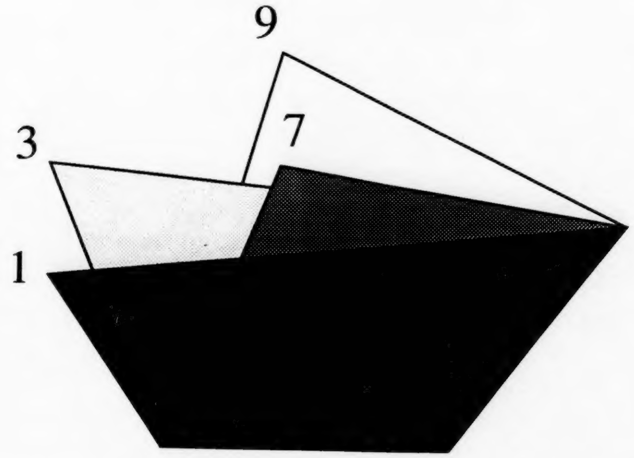


Fig. 6. The four basic shapes of the second experiment have two sides in common. They differ in the position of a single vertex (see also fig. 1).

one-half times as far), so that smaller shapes were presented on the screen. The principal change in the display concerns the *relative orientation* of the standard and comparison shapes. The term 'relative orientation' indicates that a constant angular difference was maintained as the entire display rotated about the z coordinate axis at 20°/sec. The comparison shapes stood at one of several relative orientations to the standard shape. The comparison shape lay in the same plane (the XY plane) as the standard shape, in one condition. In other conditions the comparison shape had been rotated about the x axis to make an angle between the plane of the comparison and the plane of the standard: either zero, an acute angle, or a right angle. Ten conditions of relative orientation differed by steps of 10° over a range of 90° (see fig. 7). In other respects the procedure was the same as for the first experiment, so that the present experiment has a 4×10 (SHAPE × ORIENTATION) repeated measures design.

Results and Discussion

The dependent measure for the first analysis is again the cross ratio of the comparison shape for a particular trial, minus the cross ratio of the appropriate standard shape. An analysis of variance showed a significant effect of changes in SHAPE on the dependent measure ($F(3,57) = 17.02$, $p \leqslant 0.01$), as was found in the first experiment. Mean departures in cross ratio are different from zero for two comparison SHAPEs, as shown by one-group t-tests (see table 1). No significant difference was found on the dependent measure among the ten conditions of relative angular ORIENTATION. Nor was there any significant interaction of SHAPE and ORIENTATION (fig. 8). In this experiment the angular difference between standard and comparison makes no significant contribution to the projective distortion associated with changes in shape.

These results can be explored further by plotting the discrepancy in rectangular coordinates of the comparison shapes, for each standard SHAPE at each ORIENTATION. Fig. 9 shows the pattern of these departures from the correct point. Hotelling's

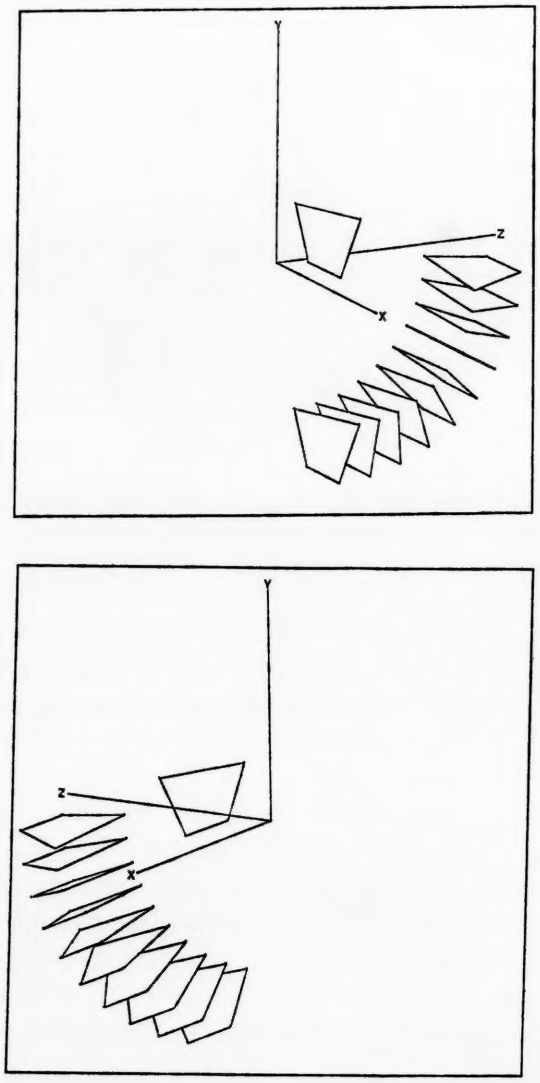


Fig. 7. The comparison shape took on several orientations to the standard shape in the second experiment. The comparison shape varied 90° in orientation from the *XY* plane to the *XZ* plane, in steps of 10°. These two frames present two views of the overall arrangement.

T^2 statistic was computed on X' and Z' departures for particular SHAPEs, to determine if the points chosen cluster about the correct point. None of them do, as shown in table 1. Estimates of both the Euclidean and projective properties of these rotating shapes depend on the shape that is to be estimated. In the present experiment such estimates are largely independent of the relative orientation of the standard and comparison shapes. It is noteworthy that differences in ORIENTATION do not produce significant effects, where differences in ORIENTATION did produce significant effects in the first experiment. This may be attributed to the *number* of operations of rotation that would be required to transform the standard shape into congruence with the comparison shape. In the first experiment two

Table 1
Departures from correct projections: Experiment 2 ($n = 200$).

Shape	Mean cross ratio	SE cross ratio	p	Hotelling's T^2	p
One	0.16	0.02	$\leqslant 0.001$	49.59	$\leqslant 0.01$
Three	−0.01	0.01	ns	13.57	$\leqslant 0.01$
Seven	−0.01	0.01	ns	14.71	$\leqslant 0.01$
Nine	−0.08	0.01	$\leqslant 0.001$	23.45	$\leqslant 0.01$

Note: Hotelling's T^2 statistics were computed on X and Y departures from the correctly projected points for each shape.

rotations are necessary, while in the second experiment a single operation of rotation suffices. While it is true that any such compound operation may be effected by a 'screw' transformation, still some rotations may be counted as more difficult than others.

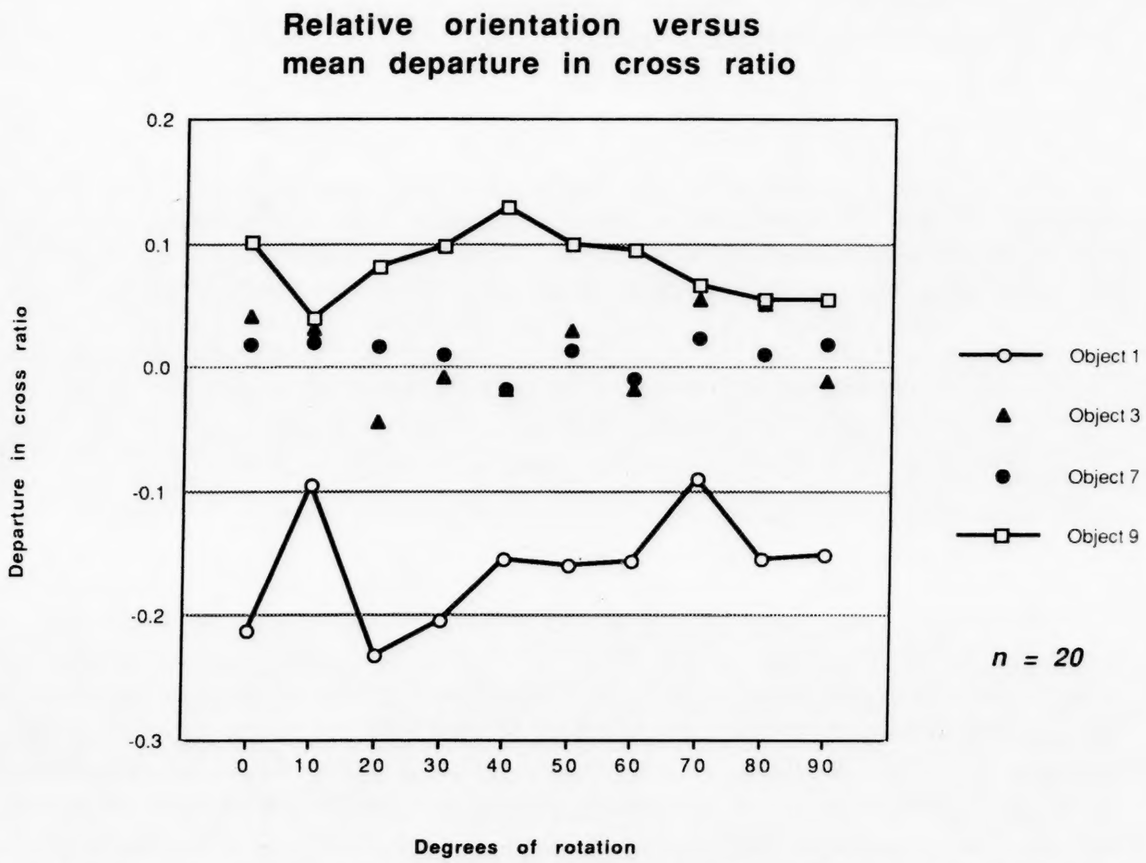


Fig. 8. If observers had estimated projective properties accurately, all points in this graph would fall along a horizontal line through zero. Yet for two standard shapes, observer's estimates are different from zero on average. Changes in relative orientation (i.e., degrees of rotation) have little effect on these errors in estimate.

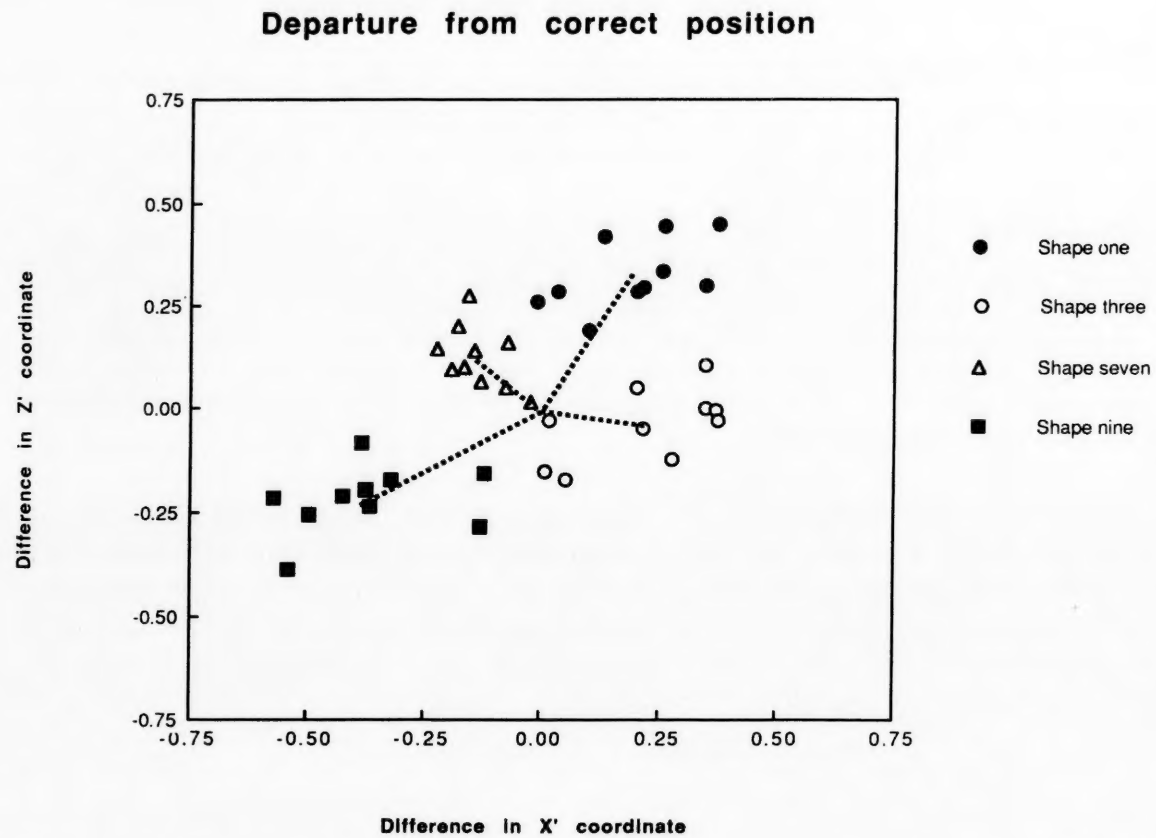


Fig. 9. Let the correct position of the adjustable vertex of the comparison shapes lie at (0,0) in this graph. X' and Z' coordinates of average departures from those positions are shown. Different symbol types indicate different standard shapes; different instances of the same symbol indicate different relative orientations of the same standard and comparison pair. Twenty observers made one estimate in each condition. Estimated positions do not cluster about the correct point; instead, they seem to cluster by shape. Dashed lines connect the correct position with average departures over conditions of relative orientation.

Experiment 2A

The previous experiments show that observers' estimates can be inaccurate in projective terms, but they do not show how accurately observers may reproduce shapes with the experimental apparatus. It could be that observers are unable to use the 'mouse' device efficiently, or it could be that observers are unable to make accurate estimates of shapes in continuous yoked rotation. Niall and Macnamara (1990: 655–656) found that observers copy these quadrilaterals very accurately, despite fixed angular differences in rotation of the figure in the picture plane. Perhaps observers reproduce these quadrilaterals accurately when they rotate continuously without change of shape in the picture plane, as well. Such accurate performance would provide a useful comparison to the conditions under which observers' matches have been shown to depart from projective equivalence.

Subjects

Three adults from DCIEM were tested. They are: observer TK (19 years of age, male); observer KN (36 years of age, male); and observer MW (22 years of age, female). Observers TK and MW were unaware of the purpose of the experiment.

Stimuli

Two planar quadrilaterals were presented on each trial: a fixed standard shape and an adjustable comparison shape. These shapes rotated continuously in the picture plane at a rate of 20°/sec; they rotated about a common point at a single rate. The point was centred on the screen. The four standard shapes of experiment 2 were used, and the viewpoint was specified to be at the same distance as in experiment 2. Each shape was embedded in a flat rectangular patch of texture, formed by two thousand luminous dots that rotated with the shape. (This anticipates the stimuli of experiment 3.) Each pair of a standard and a comparison shape was presented twenty times to each of three observers. Then the experiment has a 4×3 (SHAPE $\times$ OBSERVER) repeated measures design. Repeated responses (in twenty trials) by an observer take the place of many observers for each condition. The method and instructions were otherwise the same as for experiment 2 (there is one obvious difference: observers were no longer free to alter the viewpoint of the display).

Results and Discussion

The dependent measure is the cross ratio of the comparison shape as adjusted by the observer in a single trial, minus the value of the cross ratio for the standard shape in the trial. These departures in cross ratio are vanishingly small under the conditions of this experiment. Median departures for each shape and each observer are displayed in table 2, where quartile deviates (semi-interquartile ranges) are displayed as well.

The usual tests of significance would be uninformative here, since the median departures are small enough that they may not be distinguishable from sources of error such as display error or rounding error in calculation. It seems clear that observers can reproduce shapes accurately with the experimental apparatus, if those

Table 2
Departures from correct projections: Experiment 2A ($n = 20$).

Shape	Observer TK		Observer KN		Observer MW	
	Median dCR	Quartile dev.	Median dCR	Quartile dev.	Median dCR	Quartile dev.
One	−0.01	0.03	−0.03	0.03	0	0.03
Three	−0.03	0.02	−0.03	0.01	−0.02	0.02
Seven	0	0.01	0	0.01	−0.01	0.02
Nine	0	0.01	−0.02	0	0	0.01

Note: The median and quartile deviate (semi-interquartile range) of departures in cross ratio were computed.

shapes are presented in continuous rotation in the picture plane. As Gibson knew, constant projective properties are disambiguated from variable Euclidean properties of shape in the kinetic depth effect. Yet in this experiment, constant projective properties of shape have been confounded with constant Euclidean properties in the picture plane, to show that observers can estimate shape accurately. 'The present experiment confirms shape constancy under the conditions we have employed, and does not separate the influences of projective properties from those of Euclidean properties that will imply them' (Niall and Macnamara 1990: 656).

Experiment 3: The addition of texture

The results of experiment 2 show no significant effect of relative spatial orientation on estimates of projective properties, in the absence of a textured background to indicate the plane in which a shape lies. Gibson's conjecture is that the addition of texture helps to determine the perception of shape at a slant. Beck and Gibson (1955: 126) hypothesized that 'phenomenal shape becomes indeterminate when phenomenal slant is made indeterminate', where slant is made apparent by increasing amounts of texture. Todd and Akerstrom (1987: 242) note that Gibson assumes such textures have randomly distributed elements: they are 'stochastically regular'.

The slant and tilt of a planar shape is made apparent in the kinetic depth effect by means of motion as well as texture. Yet there is some evidence that spatial orientation does not affect estimates of textured form significantly, at least for static displays. Uttal (1983, 1985) asked observers to discern complex static forms behind static screens. The surfaces of these forms were defined by many luminous dots, and a scatter of luminous dots in the picture plane is the screen (or mask). Given a picture of all these dots, observers are asked to discern the masked form. An interesting result emerges when observers are asked to discern a plane at a slant behind the screen: for a given dot density of the plane at a slant, there is 'virtually no effect on detectability' (Uttal 1983: 108) associated with changes in the orientation of the plane. Detectability is not influenced significantly by the combined operations of slant and tilt.

Uttal's point is that the constant shape of a plane figure can be discerned, largely independent of the differences in perspective gradients of dots that change from picture to picture. The present display is different from Uttal's in that here the texture is bound to the moving plane of the object. Still, Uttal's results raise a general question about texture gradients and slant. The question is: Will the addition of texture, or a larger range of differences in spatial orientation make a difference to observers' estimates of shape?

Method

Subjects

Twenty students and employees of DCIEM were tested. These observers had not participated in the previous studies. Seven were men and thirteen were women; the

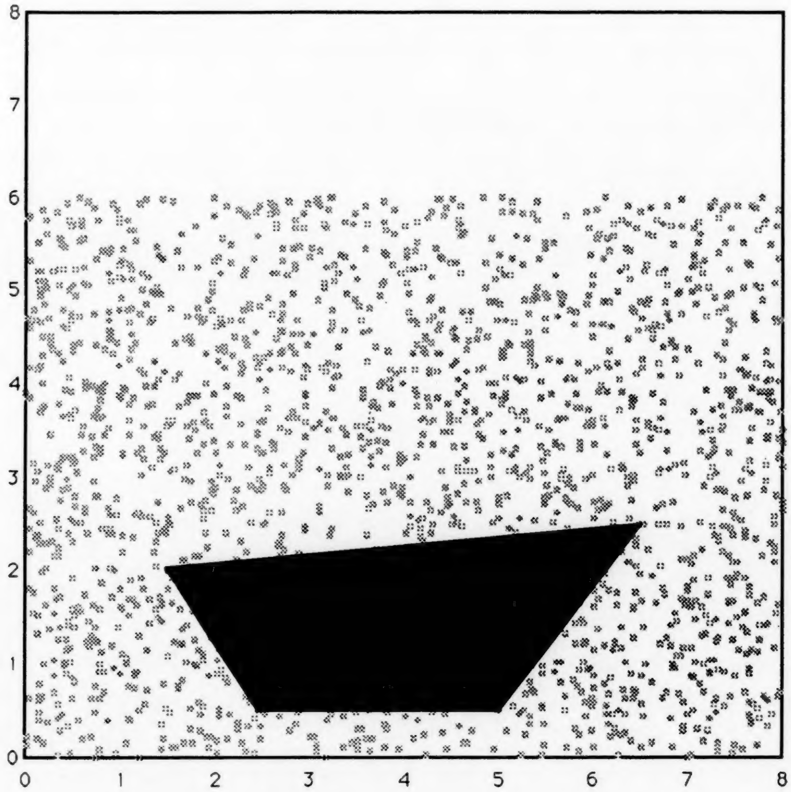


Fig. 10. A textured ground was added to both the standard and comparison shapes of the third experiment. Two thousand dots were arranged randomly within a rectangle that enclosed the shape. The dots helped to indicate the orientation of the plane in which the shape was embedded.

mean age of these observers was 25.5 years. Eleven observers had normal uncorrected acuity; the remainder all had acuity corrected to normal.

Stimuli

The same four quadrilaterals that had been used in the second experiment were used in the third experiment (see fig. 6). Most other features of the display were unchanged from the second experiment. Two changes were the addition of textured patches around the shapes, and different relative orientations of the standards and comparisons. Each shape, coloured green, was embedded in a patch of two thousand randomly arranged luminous green dots. Fig. 10 indicates the size of the patch in comparison to the quadrilateral shape. The adjustable vertex of the comparison shape could be positioned anywhere within the patch of dots, but not beyond it. The comparison shape was oriented at one of eleven angles to the standard shape, up to 180°. Different comparison shapes were separated by 18° over the 180° range (see fig. 11). The entire display rotated at a constant speed of 6.5°/sec about the z axis. The procedure and instructions to observers were unchanged.

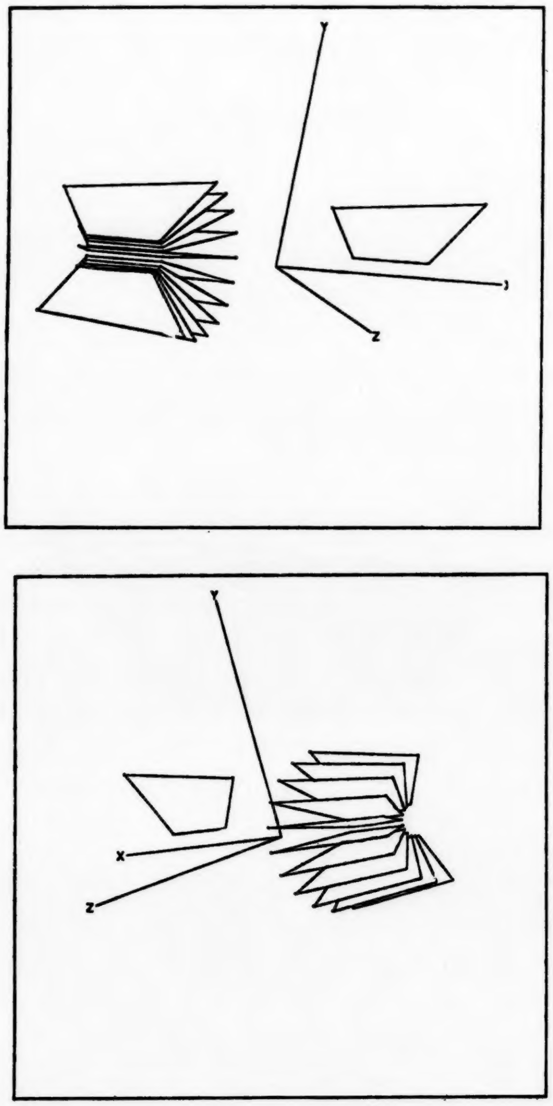


Fig. 11. The comparison shape assumed several orientations to the standard shape in the third experiment. The comparison shape varied 180° from upright to inverted, in steps of 18°. These two frames present two views of the overall arrangement.

Results and Discussion

An analysis of variance was conducted on departures in cross ratio, as in the previous experiments. Two outlying departures in cross ratio were removed from the 880 observations in the data set (they were an order of magnitude larger than the other values, and were due to two different observers). The effect of change in SHAPE on the dependent measure was significant ($F(3,57) = 5.88$, $p \leqslant 0.05$). Mean departures in cross ratio were different from zero for three of the comparison SHAPEs, as shown by one-group t-tests (see table 3). The full 180° change in ORIENTATION did not produce a significant effect. The trend of these departures

Table 3

Departures from correct projections: Experiment 3 ($n = 220$).

Shape	Mean cross ratio	SE cross ratio	p	Hotelling's T^2	p
One	0.29	0.08	$\leqslant 0.001$	15.81	$\leqslant 0.01$
Three	0.13	0.04	$\leqslant 0.001$	9.06	$\leqslant 0.01$
Seven	0.10	0.02	$\leqslant 0.001$	32.16	$\leqslant 0.01$
Nine	0.03	0.03	ns	2.59	ns

Note: Hotelling's T^2 statistics were computed on X and Y departures from the correctly projected points for each shape.

is similar to that found in the second experiment, though a direct comparison of conditions of texture is deferred until the next experiment. In projective terms, though several means of estimates were different from zero, the means of estimates were not significantly different one from another across conditions of ORIENTATION. Hotelling's T^2 statistic was computed on X' and Z' for particular SHAPEs, to determine if the points cluster about the correct position. Three of the four groups of points do not, as shown by significantly large T^2 statistics ($p \leqslant 0.01$).

The addition of a fixed amount of texture to the backgrounds does not produce a marked improvement in projective terms to estimates of shape, which is to say that it does not have as large an effect on estimates of shape as do differences in shape itself. Perhaps significant effects will emerge when different amounts of texture are added, as in the next experiment.

Experiment 3A

Both speed and texture have been cited as conditions for shape constancy in the kinetic depth effect. That is, a great amount of texture or a moderate amount of rotation have been considered to be conditions that trigger the perception of constant shape (Epstein and Park 1964). Then the accuracy with which shapes are estimated through vision should be affected when these conditions are altered. For example, Loomis and Eby (1988) have reported conditions under which the motion of figures may lead to departures from shape constancy. (They seek to establish that such departures are contingent upon object motion, and do not relate their findings to projective measures.) The present experiment varies the number of randomly arranged dots that form a textured background for the shapes, while the display's speed of rotation is maintained. And conversely, display speed is varied while texture density is maintained. Differences due to texture or display speed might help to account for differences in the absolute magnitudes of departures from correct cross

ratio between experiments 2 and 3. Do the projective properties of observers' estimates vary with the speed of rotation of a display, and do they vary with the accumulation of texture in the background planes?

Subjects

Three adults from DCIEM were tested. They are: observer BF (22 years of age, male); observer TF (23 years of age, female), and observer JD (22 years of age, male).

Method

Most features of the display were unchanged from the third experiment. The number of dots was varied in the texture patches. Some shapes had no dots for their background. In other conditions, either one thousand, two thousand, three thousand, or four thousand dots formed the background of each shape. The planar patch of dots rotated together with the shape so that shape and dots moved as a unit. Each of these displays rotated as a whole about the z axis at one of two speeds: 20°/sec or 40°/sec. Differences in the number of dots in each textured patch gave rise to a factor with five levels, DENSITY, and differences among the four shapes used in the experiment give rise to another factor: SHAPE. The two rates of rotation form a factor with two levels, SPEED, and differences among the three observers form another factor, OBSERVER. Thus the experiment has a four-way ($5 \times 4 \times 2 \times 3$) repeated measures design. Repeated responses (in fifteen trials) by an individual take the place of responses of many observers for each condition. The procedure and instructions to observers were unchanged from experiment 3.

Results and Discussion

As in experiment 1A, the unit of observation in this experiment is a single trial, rather than a single observer. The four-way analysis of variance on departures in cross ratio showed just two effects. There was a significant effect of differences in SHAPE ($F(3,42) = 176.67$, $p \leqslant 0.01$), and there was a significant interaction of SHAPE and OBSERVER ($F(6,84) = 38.90$, $p \leqslant 0.01$). In this experiment the speed of rotation of the display and the density of a textured background make no significant contribution to the projective distortion associated with changes in shape and individual differences. The grand mean of departures in cross ratio was significantly different from zero ($n = 1800$, $p \leqslant 0.001$), as were overall departures in X and Y coordinates of estimates from the correct point (Hotelling's $T^2 = 724.08$, $p \leqslant 0.001$) Each of the observers showed at least three significant differences from zero in mean departure from the correct values of cross ratio, for the four SHAPEs (all $p \leqslant 0.01$, $n = 150$).

Whatever effects may be attributable to speed and density of texture, they are not significant under the conditions of the present experiment. Instead, they are subordinate to individual differences of estimate and the effects of differences in shape. Are these results restricted to the planar shapes that have been displayed, or will they generalize to solid forms, as well?

Experiment 4: Solid forms and changes in orientation

Observers discern solid forms with ease, even in the absence of effective binocular disparity. The aim of the present experiment is to begin to determine how well observers come to discern novel solids. It might seem that the *purpose* of vision is to reconstitute solid form out of planar images, since retinal images can be considered flat, and since far-distant objects may appear as flat as pictures. Yet this assumes correct projections of solid objects are *given* to the visual system, in contrast to the way in which solid forms are said not to be present or given in a flat retinal image. The reflection and propagation of light to the eyes is aptly described as the geometric projection of solid forms onto surfaces. However, this elementary principle of optics has led psychologists to claim that the laws of projection wholly govern the appearance of solid forms (Caelli (1979: 29) would have solid objects perceived 'as central projections' whereas it would be more accurate to say that the laws pose initial constraints on visual form perception). Hence the perception of solid form itself has been disregarded, in favour of emphasis on rules of projection from solids to surfaces. As late as 1981, Irvin Rock and his co-workers could say: 'to our knowledge the fundamental question of three-dimensional visual form perception has never been directly investigated' (1981: 723). The following experiment applies a measure that is novel to vision research, to investigate the perception of solid forms in motion.

Method

Subjects

The twenty observers were the same ones who participated in experiments 1 and 2.

Dependent measure

How are the projective properties of solid forms to be measured? A new dependent measure is introduced for this analysis: the cross ratio of points in space. This cross ratio is an absolute invariant of six points, of which no four are coplanar. The cross ratio of points in space remains constant in value, should all six points be subjected to the same linear transformation. There are conditions under which such a transformation may be experienced, as when one moves one's head some distance from side to side while viewing a nearby stereogram.

Another operation is more interesting for its application to vision: that is the projection of a solid form onto a picture plane. It can be shown that the value of the cross ratio of points in a solid form is equal to the value of the cross ratio in the plane, for what is known as a *dual* configuration (see Busemann and Kelly 1953: 238–244). However, let us take the cross ratio of points in space simply as an example of a projective property of solid forms. (One may also consider that a mapping of a three-dimensional space onto a plane induces a projective geometry of the plane subordinate to the projective geometry of the space.)

We leave aside the question of the relation between this cross ratio and projective magnitudes in the image that is cast onto a picture plane by a solid form. We consider only the projective properties of the solid form which are constant under operations

of strain and shear. The cross ratio of points in space indicates this constancy of solid form under linear transformation, at least. The method by which this quantity is calculated is set out in note [2].

Stimuli

Four basic forms were presented to observers. These solid forms are prisms (not tetrahedra): their bases and their tops are triangles which lie in parallel planes in space. The base triangle is a solid patch of colour, while the top triangle is outlined, ie., it is a 'wire-frame' shape. The vertices of these parallel triangles are joined by thin links, so that the overall form resembles a thick slab with irregular edges. These triangles are roughly centred one on another, but their vertices are not aligned in a simple way. Each vertex of the base of a form is connected by line segments to two adjacent vertices of the top of the form. Each of the four forms has the same triangle as its base, and each has a different triangle as its top. Fig. 12 shows the four forms in an orthographic projection (top view). There the top triangle is pictured in dark gray, and the base triangle is pictured in light gray.

Each display contains a standard form and a comparison form. The standard is a fixed form, while the comparison is a form that can be altered by the observer. The top of the comparison form has an adjustable vertex: its position can be varied in two directions within one plane. The position of the adjustable vertex is yoked to distal and lateral movements of the computer 'mouse'. When the adjustable vertex changes its position, the triangular top of the comparison changes in shape. The line segments that connect the adjustable vertex to vertices of the base triangle lengthen, so that the comparison remains connected, and yet changes in solid form.

The relative orientation of the standard and comparison forms was varied as in experiment 2. Here the term 'relative orientation' indicates that a constant angular difference is maintained between the bases of the two solids, as the entire display rotates about the z coordinate axis at 20°/sec. The bases of the comparison forms stand at one of several relative orientations to the bases of the standard forms. The base of the comparison form lies in the same plane (the XY plane) as the base of the standard form, in one condition. In other conditions (of the factor ORIENTATION) the comparison shape is rotated a fixed angle about the x coordinate axis, to make an angle between the plane of the base of the comparison form and the plane of the base of the standard form. This angle ranges from 0° to 90°, that is, ten conditions of relative orientation differ by steps of 10° over a range of 90°. Then the experiment has a 4×10 (FORM $\times$ ORIENTATION) repeated measures design.

Results and Discussion

The dependent measures for the initial analysis are three different cross ratios of points in space: CR_{14}, CR_{24}, and CR_{34}. Values of these three cross ratios in space, calculated on the standard FORMs, are subtracted from the corresponding three values of cross ratio calculated on the comparison form as adjusted by the observer. If an observer were able to reproduce the projective properties of solid form, then all the dependent measures would be zero on average in all conditions of the experiment. A multivariate analysis of variance (MANOVA) shows a significant effect of FORM

on the dependent measures ($F(9,1844) = 16.24$, Wilks's λ criterion, $p \leqslant 0.01$). No significant differences were found among the ten conditions of ORIENTATION. Nor were there any significant interactions between the effects of FORM and ORIENTATION. Similar results emerge if two other dependent variables are evaluated that may be more familiar to the reader: X and Y coordinate departures of the adjusted position of the cursor from its correct position, where the cursor is constrained to lie

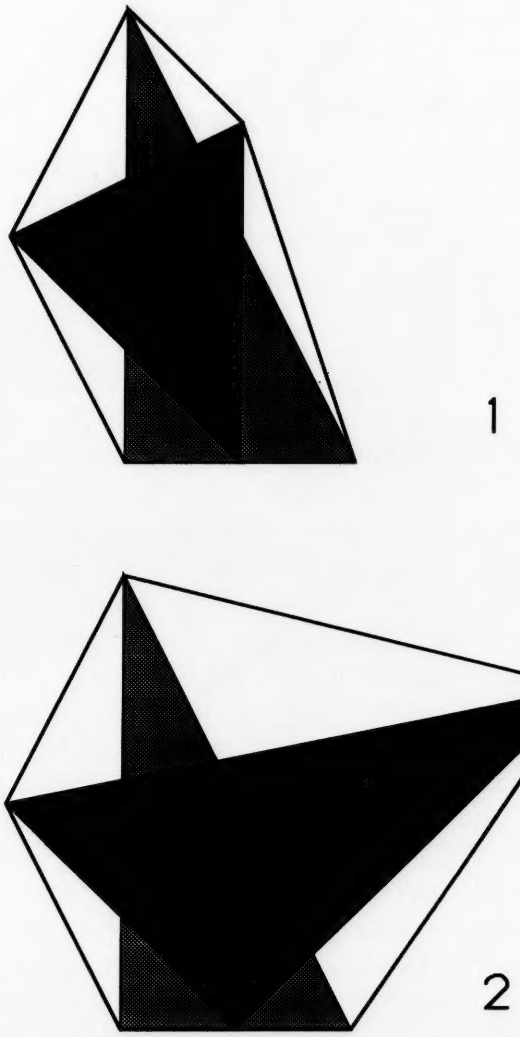
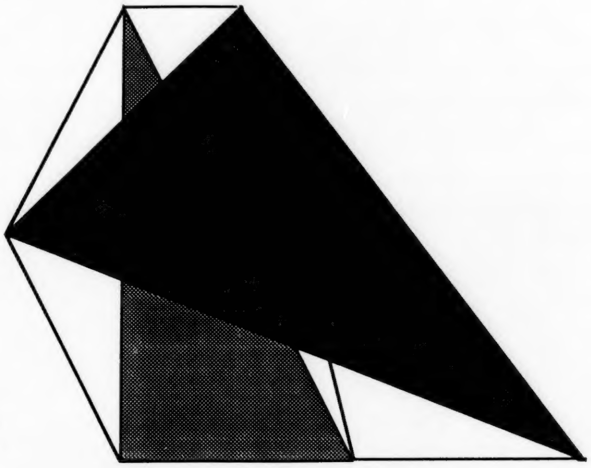


Fig. 12. The solid forms of the fourth experiment are four prisms (ie., not tetrahedra) that have triangular tops and bases. The top of each solid is a plane triangle parallel to the base triangle. The nearest vertices of the two triangles are connected by thin bars. These solid forms are difficult to discern in orthographic projections such as this, or even perspective projections, but their form becomes conspicuous as soon as they are pictured to rotate in depth. Static perspective views from oblique angles tend to give an obscure rather than a clear impression of these solids. The coordinates of the vertices of the top triangle are:
1: (2,0,2),(2,3,2),(0,2,2); *2*: (2,0,2),(5,3,2),(0,2,2); *3*: (5,0,2),(2,4,2),(0,2,2); *4*: (5,0,2),(5,4,2),(0,2,2).
The base triangles lie on the plane for which $z = 0$.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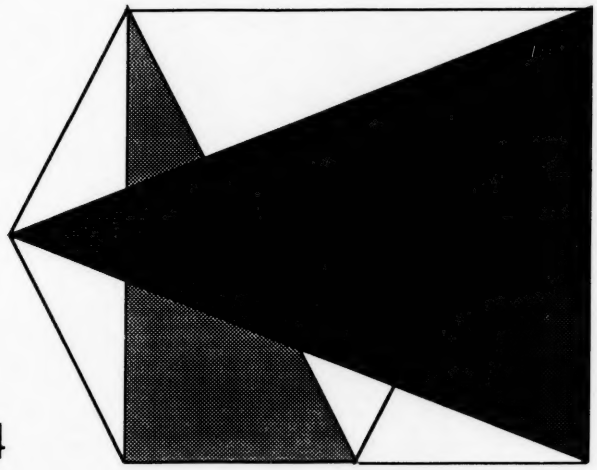
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Fig. 12. (continued)

in the top plane of the solid form. Here again MANOVA shows a significant effect of FORM ($F(6,1518) = 26.73$, $p \leqslant 0.01$), no significant effect of ORIENTATION, and no significant interaction of FORM and ORIENTATION. It is important to know that the effects of different FORM represent significant differences from zero in these cross ratios. A difference can be shown by computing an F statistic for the three cross ratios, or by Hotelling's T^2 statistic for X and Y departures. These differences are significant in the case of each FORM, as shown in table 4.

Differences of the cross ratios from their expected values are graphed as fig. 13. Changes in relative orientation have little effect on the estimates, as can be seen in fig. 14. It may be noticed that departures are generally greater for CR_{34} than for the other cross ratios. In part this may be attributed to constraints on cursor movement. Observers were able to move the cursor in only one plane of the object. If given more freedom, they might have chosen to move the cursor outside that plane.

Table 4

Departures from correct projections. Experiment 4 ($n = 200$).

Solid form	Mean CR 14 Mean CR 24 Mean CR 34	F	p	Hotelling's T^2	p
One	0.03 −0.03 −0.29	9.94	$\leqslant 0.01$	15.80	$\leqslant 0.01$
Two	0.06 0.02 −0.41	27.24	$\leqslant 0.01$	39.67	$\leqslant 0.01$
Three	−0.36 −0.07 −0.37	15.01	$\leqslant 0.01$	43.22	$\leqslant 0.01$
Four	0.15 0.08 −0.19	14.55	$\leqslant 0.01$	14.39	$\leqslant 0.01$

Note: Hotelling's T^2 statistics were computed on X and Y departures from the correctly projected points in the base plane of each solid form.

The results of experiment 4 replicate those of experiment 2; they indicate that differences in shape or form lead to significant departures in the projective properties of estimates of shape. These effects are not influenced as much by differences in the spatial orientation of the shapes, when differences in spatial orientation can be described by a simple rotation about a single coordinate axis.

General discussion and conclusions

'Quelle démonstration de la force géométrique que l'angle de lumière, les branches mobiles et rigoureusement immobiles du compas qu'étaient ses deux jambes quand il marchait!' (Genet 1953: 89)

The main finding of the experiments is that observers' estimates of projective properties are often biased, to a large fraction of cross ratio. The continuous motion of objects does not alleviate the departures from correct projection that have been found in observers' estimates of static shapes. Moreover, motion disambiguates persistent errors in projective properties from errors in accessory Euclidean properties in the picture plane. The evidence of the experiments

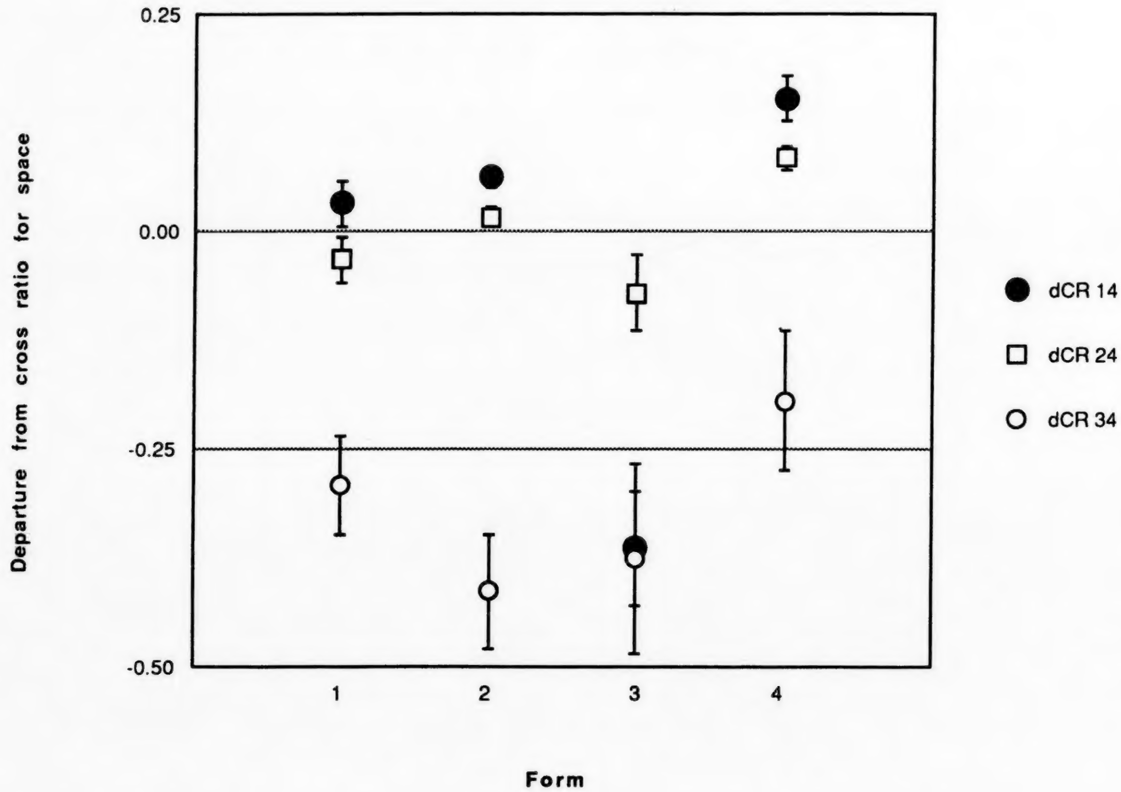

Fig. 13. Estimates of the cross ratio of points in space depart from their expected values in the fourth experiment. Three cross ratios are plotted for each solid form. Standard error bars are indicated.

indicates a persistent effect of differences in shape, and comparatively little or no effect of texture, speed of rotation, or relative orientation about a single axis.

Let us discuss this evidence. The first experiment establishes a basic effect: that there is a reliable difference between true cross ratio and estimated cross ratio for some shapes in continuous rotation. There is a linear trend between cross ratio and error of estimate in cross ratio, though it remains to be seen whether this trend remains when data for different shapes that have the same cross ratio are included. Another finding of the first experiment is that there are small but reliable differences in estimated cross ratio, that depend on the coordinate plane in which the comparison shape lies. Note that in this experiment the comparison shape must be rotated about more than one coordinate axis to coincide with the standard shape. Experiment 1A shows that these departures from correct cross ratio are not exacerbated by the yoked rotation of the comparison shape. That is, when a static

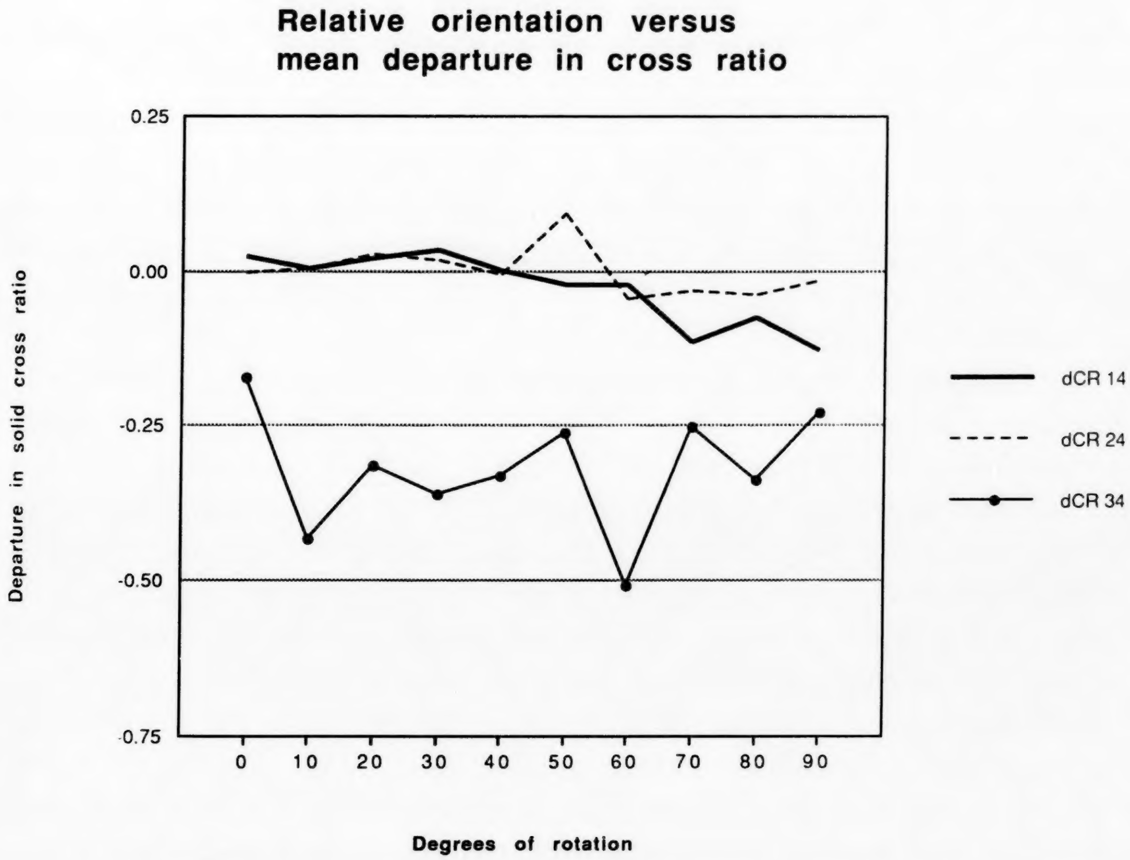

Fig. 14. If observers' adjustments represented unbiased estimates of cross ratios, all points in this graph would fall along a horizontal line through zero. Yet for one measure of cross ratio, observers' estimates fall consistently below the expected value. Changes in relative orientation have little effect on these estimates. The term 'dCR' indicates departure in cross ratio for points in space.

comparison shape is placed in the picture plane, left–right reversals occur among estimates of shape, and estimates are no more accurate than those obtained when the comparison shape is in motion. The results of experiment 2 are in accord with those of the first experiment: errors in estimate of the cross ratio depend upon differences in the standard shape. Surprisingly in this experiment, a 90° difference in relative orientation did not alter the magnitude of error in cross ratio significantly, even for individual shapes. The results of experiment 2A indicate that errors of this magnitude are not present for shapes that rotate in the picture plane. The third experiment extends the main results to shapes that are embedded in dense patches of texture. Again, errors in estimate of cross ratio occur with differences in the standard shape. Neither the effect of relative orientation – where

difference in relative orientation can be as large as 180° about a single coordinate axis – nor its interaction with the effect of shape is significant statistically. Experiment 3A extends this finding to show significant effects of difference in shape and significant individual differences, where large changes in the amount of texture of the display and a 20°/sec change in the speed of rotation of the display produce no significant differences in the projective properties of estimates.

Experiment 4 shows that observers do not perform significantly worse when comparing complex solids at different relative orientations, than when comparing them side by side. Continuous rotation makes the reoriented solids recognizable, and observers' errors in reproduction are roughly constant despite changes in relative orientation. The exact pattern of results may be influenced by a restriction of range in the dependent variables, that is imposed by the few degrees of freedom in the task given to observers. The combined results of the experiments are univocal: differences in shape give rise to different errors in cross ratio, and texture density, rate of motion, and relative orientation affect these errors comparatively little. There is a significant effect of figural orientation when the standard and comparison figures are separated by more than one operation of rotation about the coordinate axes.

These results are the third step in an argument by the enumeration of cases (the first steps were Niall and Macnamara 1989 and 1990). Our aim is to show that significant departures from projective accuracy can be demonstrated in many conditions of observation, under different task demands. Again, 'we have not proved once and for all that the projective thesis never provides an explanation of shape constancy, since, naturally, we have not examined all possible conditions in which the thesis might obtain' (1990: 658).

Many psychologists have conceived form perception as an unconscious process of geometric construction or geometric proof. Like Poggio and Koch (1985: 303; also Reuman and Hoffman 1986: 210), they see shape constancy as the resolution of an 'inverse optics' problem. Although a machine might be built to retrodict the solid geometry of a landscape given various perspective views, what impels psychologists to call this an analogy to vision? Surely not the propagation of light alone. Inverse optics (or 'natural geometry') offers no explanation of vision but to say that things cannot appear to be

otherwise than they are, when viewing conditions are taken into account. The inverse optics problem is a way of *conceiving* visual form perception; it precedes rather than follows empirical inquiry. One possibility is that inverse optics is a *false* conception of the problem of visual form perception.

'Inverse optics' does not constitute a system of laws for the visual perception of form, and this is a conceptual matter, not just an empirical matter. Psychologists like Rock (1983: 324; 1985: 18) suppose that laws of visual form perception can be based on laws of the propagation of light. Such notions have the ring of truth about them only because perceptual concepts are introduced under normal optical conditions (see Hacker 1987: 128). Dioptrics merely describes some *background conditions* of the visual perception of form; the psychologically relevant background conditions must be specified in terms of perceptual concepts (among others). That is, these conditions come to be identified for the reason that they are the ones under which shape constancy obtains.

Geometrical optics does not provide explanation of shape constancy and the kinetic depth effect, any more than a description of light, water, and soil explains the normal growth of plants. (That is to say geometrical optics constrains vision just as the availability of water constrains plant growth – and the study of water on its own does not elucidate plant growth.) Yet Ryle (1949: 326) considers that shape constancy needs no additional psychological explanation. He says: 'We cannot, from our own knowledge, tell why a straight line cutting through certain cross-hatchings looks bent ..., and we recognize [this for a psychological question]. Yet we feel that the wrong sort of promise is being made when we are offered corresponding psychological explanations of our correct estimations of shape, size, illumination, and speed. Let the psychologist tell us why we are deceived; but we can tell ourselves and him why we are not deceived.' So long as psychological explanation emphasizes dioptrics, Ryle's point is well taken.

Geometrical explanations of the kinetic depth effect have an affinity with the traditional story that seeing a circular plate at a slant is a matter of having an elliptical image (on the retina, in an idealized lens image, in the camera obscura or elsewhere). The plate is seen to be circular when the retinal image changes, that is, as the object moves relative to the observer. The effect is supposed to have an explanation

in the unchanging geometric properties of the image, though according to Ryle's conjecture no explanation of shape constancy is called for. And where observers' estimates do depart from shape constancy, their estimates do not reflect the unchanging geometric properties in question, as the experiments show.

Such geometrical accounts of the kinetic depth effect are stopgaps of explanation, tidy solutions designed to circumvent difficult problems in the intentionality of visual perception (see note [3]). 'Inverse optics' is a poor substitute for an intentional account of visual form perception. Before these difficult problems are resolved, at least we have an optical technique that is available for assessing departures from shape constancy: we can use the value of absolute projective invariants as scales of measure for the estimation of form by eye (note [4]). Gibson seems to have understood this point well, though he did not submit his strong assertions to an effective empirical test. The optical technique is a means, not an end of investigation. In that much the technique is like a ladder by which we may ascend to a better understanding of form perception, though the ladder may be thrown down after it is no longer needed. As Fodor and Pylyshyn (1981: 194–195) remark in their assessment of Gibson's ecological optics: 'To summarize: Even if all you want is to construct a theory of perception, you cannot do much without encountering problems about intentionality ...'. So may we come to know that there is more to the kinetic depth effect than the dim play of shadows.

Notes

[1] How can projective invariants form the basis of an optical standard for assessing departures from shape constancy, if accurate perception of projective invariance is not the basis of shape constancy? One need only look to colorimetry for an analogy. The analogy illustrates, perhaps incompletely, how a serviceable description of physical conditions in a domain does not constitute a psychological theory of perception in that domain.

Some simple colorimetric systems such as Maxwell's colour triangle are based on the effects of mixtures of lights. These systems depend upon the *trichromatic generalization*, which states that 'many color stimuli can be matched in color completely by additive mixtures of three fixed primary stimuli whose radiant powers have been suitably adjusted' (Wyszecki and Stiles 1982: 117). The description of a mixture of stimuli – which means here the mixture of lights – is a description of some physical properties of light, as is description of the projective properties of light in a scene. But the subject matter of colorimetry is not simply the mixture of lights. A different topic in colorimetry is the development of a system for arranging just-perceptible differences in colour. (The psychological theory of uniform chro-

maticity scales subsumes the problem how one may derive a *line element* for colour space.) The notion of a perceived difference in colour is a psychological notion, much like the notion of perceived difference in shape is a psychological notion. The point is that in colorimetry, specification of a mixture of lights is a different matter than the elucidation of perceived differences in colour.

So too in the study of form perception, specification of the projective properties of extended lights is a different matter than the elucidation of perceived differences in shape. The physical conditions of colour perception (as expressed in the tristimulus generalization) are described in a different way than a psychological problem of colour perception (that of determining a line element for uniform chromaticity space, in the modern paradigm). Following the analogy, the physical conditions of form perception (as expressed in terms of projective properties) can be described in a different way than the psychological problems of form perception. One may keep in mind that, like the trichromatic generalization of colorimetry, the assessment of departures from shape constancy by means of projective invariants *begins* a psychological investigation; the investigation does not end at that point.

[2] *Cross ratio of points in space*

How is the cross ratio of points in space calculated? The method of calculation is best expressed in terms of matrix algebra, specifically with the determinants of matrices of coordinates. The mathematician Felix Klein puts it this way:

'In our examples, we have always reached our invariants by setting up determinants, and in this we find justification for the *theory of determinants as the foundation for the theory of invariants*. Because of this connection, Cayley originally used the name hyperdeterminants for invariants. It was Sylvester who introduced the word invariant'. (Klein 1939: 143)

Cartesian coordinates of points may be arranged in matrix form, and geometric invariants can be expressed as the determinants of those matrices. The complete system of invariants associated with the points consists of such determinants (Klein 1939: 144). Let the coordinates of a point A in space take the following matrix form: $[X_A \;\; Y_A \;\; Z_A \;\; 1]$. The number one is included in the original coordinates for convenience; the extra term will simplify the interpretation of coordinates in terms of projective geometry. Such augmented coordinates are known as 'homogeneous coordinates'.

The determinant of the square matrix for four points in space

$$\omega = \begin{bmatrix} X_A & Y_A & Z_A & 1 \\ X_B & Y_B & Z_B & 1 \\ X_C & Y_C & Z_C & 1 \\ X_D & Y_D & Z_D & 1 \end{bmatrix}. \tag{1}$$

is six times the volume of the tetrahedron bounded by those points (Klein 1939: 3).

The cross ratio of points in space is defined for six points, no four of which are coplanar. The cross ratio of points in space is an absolute invariant of the six points; it is unaffected by any linear transformation, such as shear or strain, on the six points in space. (Veblen 1918: 56. The following treatment may be found there.) This cross ratio can be thought to apply to two variable points, together with a *tetrahedron of reference* whose vertices are fixed by the remaining four points. This cross ratio $\mathrm{CR}_{\text{three}}$ of points in space is related to the cross ratio CR_{one} of four points on a line. Namely the $\mathrm{CR}_{\text{three}}$ of two variable points and the tetrahedron of reference is the CR_{one} of those two variable points along the line they determine, and two points in which that line meets the tetrahedron of reference (i.e., it meets the planes which contain the faces of the tetrahedron of reference).

The cross ratio of points in space can be expressed in matrix terms. Square matrices are formed of the homogeneous coordinates of six points in space, taken four at a time. Four such matrices are constructed for each cross ratio. The homogeneous coordinates of four points are given in (1); the homogeneous coordinates of two additional points are given by: $[X_M \; Y_M \; Z_M \; 1]$ and $[X_N \; Y_N \; Z_N \; 1]$. Consider the following matrix, which is obtained by substituting the coordinates of point M for those of D in (1):

$$\begin{bmatrix} X_A & Y_A & Z_A & 1 \\ X_B & Y_B & Z_B & 1 \\ X_C & Y_C & Z_C & 1 \\ X_M & Y_M & Z_M & 1 \end{bmatrix}. \tag{2}$$

Abbreviate the determinant of this matrix by naming the rows as $|A\;B\;C\;M|$. (The reader is reminded that the determinant of the transpose of a matrix is equal to the determinant of the original matrix.) Another of the four matrices may be obtained by substituting the coordinates of N for those of A in (1); the determinant of this matrix can be written $|N\;B\;C\;D|$. Then one cross ratio can be written as:

$$\mathrm{CR}_{14} = \frac{|M\,B\,C\,D|}{|A\,B\,C\,M|} \div \frac{|N\,B\,C\,D|}{|A\,B\,C\,N|}. \tag{3}$$

Similarly,

$$\mathrm{CR}_{24} = \frac{|A\,M\,C\,D|}{|A\,B\,C\,M|} \div \frac{|A\,N\,C\,D|}{|A\,B\,C\,N|},$$

and

$$\mathrm{CR}_{34} = \frac{|A\,B\,M\,D|}{|A\,B\,C\,M|} \div \frac{|A\,B\,N\,D|}{|A\,B\,C\,N|}.$$

Since these quantities have an interpretation as the cross ratio among four points on a line (Veblen 1918: 56), they enter into the same relations as do cross ratios of points on a line.

[3] What could it mean to say that vision is intentional? Among other things, it is to affirm that we may see things we can bump into. People who can see become acquainted with their surroundings by means of vision; we say they come to know about the world by looking. That, after all, is the aim of vision and the explanandum of any account of the intentionality of vision. One of the first questions Aristotle poses in his treatise in psychology, *De Anima*, is: 'Why do we not perceive the senses themselves, as well as the external objects of sense?' (417a). He may be construed as asking: 'How do we see much of anything past corneal imperfections, detritus in the vitreous humor, an uneven arrangement of retinal elements, and a weak topographic mapping of retinal elements to the visual cortex?' Aristotle does not give a convincing answer, but his question is astute. What is seen is distinct from the observer, or at worst is experienced as distinct from the observer. And the point is surely correct: we do see many things we can bump into. This is signalled by the very word 'intentional', which has its roots in the verb '*intendere*' (to strain towards).

Vision shares characteristics of intentionality with domains of cognition such as belief and hope. Suppose that on a baseball field, a batter remarks: 'What I saw when the ball whizzed

by was a white blur' (the example is adapted from Hacker 1987: 33). What went by was a softball with a definite outline. What is remarkable here is that though a softball is not a white blur, one can correctly say that in seeing it pass one saw a white blur. The batter is not committed to the truth of '*I saw a ball with a sharp outline pass by*'. Call this characteristic the *failure of true description*. This is not simply a matter of language; the ball does look a white blur. The situation is different for sentences that describe physical actions. If a shortstop catches a speeding ball and the ball has a sharp outline, then the shortstop catches a ball with a sharp outline. Chisholm (1957: 171–172) lists three distinguishing characteristics of intentionality, but singles out the failure of true description as the mark of intentionality in perception. How can we explain this characteristic of vision, by which we know vision to be intentional? (I will not provide an answer to this question here. Instead I will discuss one attempt to provide an answer.)

The blurry appearance of a speeding softball has an explanation that begins with the optics of the eye, that is, with dioptrics. A patch of light falling upon the batter's retina may be blurred, not distinct, in form. Should objects not look the way they are, traditional explanations make appeal first to the conditions under which light reaches the retina, since these conditions determine how clearly objects may be seen. Yet the success of such explanations might lead one to propose that failures of true description can be described wholly as dioptric effects. This proposal is part of the causal theory of perception according to which sight is achieved by a causal sequence or progression, beginning with the action of light and ending in a percept (whatever may be meant by 'percept'. Percepts are no more physical states than *hikes* or *jobs* are; the term 'percept' is a nominalized form of the verb 'to perceive', just as 'a hike' is a nominalized form of 'to hike'). The failure of true description is then considered to be the result of inappropriate or incomplete compensation for such 'distortions' as are introduced by dioptrics (or else retinal effects, and so on indefinitely into neurophysiology). Or else, with Landy (1987: 864) we may hold that the 'minutiae of visual optics' such as chromatic aberration are causes of a '3D percept', and that this is what is perceived. The problem with these proposals are that they assume we do perceive the senses themselves, be that in the form of retinal images, or 'percepts'. And only a small step separates the claim that we do perceive the senses from the claim that we perceive nothing else but the senses. In other words, the proposed explanation misses the point of Aristotle's question.

Though the effects of viewing conditions obtrude on vision, explanation of those effects is no explanation how we come to know the world by looking. Though viewing conditions help to explain how a softball may appear blurry, compensation for dioptrics is no substitute for explanation in terms of intentionality. Simply put, a process of compensation for dioptrics is no explanation how we see softballs, or anything else for that matter.

[4] *Relations among cross ratios*

Where the fundamentals of projective geometry are applied to the study of vision, some writers are misled because they fixate on details of technique. They tend to fixate conventions that serve to introduce projective properties. For example, one might insist that the analogue of cross ratio in two dimensions is a measure applicable to four points only, since it has been applied to quadrilaterals. That would show an unduly narrow view of the application of projective geometry. In this vein, Cutting (1986: 115) says of the cross ratio of points on a line: 'The problem is that the number of ratios burgeons exponentially as the number of elements increases linearly. Thus, for example, if there are 12 collinear elements and one of them moves, there are 165 ratios in flux. The utility of cross ratios seems diminished in such a situation. Parsimony suggests that some other information be used.'

Cutting does not mention that cross ratios which can be measured *independently* are fewer in number. Let us consider the simpler case of five points $\{x_1, x_2, x_3, x_4, x_5\}$ on a line. Define the cross ratio

$$\text{CR}1234 = ((x_1 - x_2)(x_3 - x_4))/((x_2 - x_3)(x_4 - x_1)).$$

Then the simple relations among cross ratios on the same five points are:

CR1234 = (CR5234)(CR1254) = (CR1534)(CR5321)

CR2143 = (CR2543)(CR2145) = (CR5143)(CR3512)

CR3412 = (CR3452)(CR5412) = (CR3415)(CR2153)

CR4321 = (CR4325)(CR4521) = (CR4351)(CR1235).

Other relations are obtained by permuting the labels of points. Then as a function of the point $\{x_5\}$ and three of the points $\{x_1, x_2, x_3, x_4\}$, the cross ratio CR1234 can be obtained as the product of 32 different pairs of cross ratios on the points. These relations can be used to show that no more than three cross ratios can be measured independently on five points (and that all the other cross ratios can be recovered from these, once their relations are known). Now let us consider the case of six points. As a function of the points $\{x_5, x_6\}$ and two of the points $\{x_1, x_2, x_3, x_4\}$, there are twelve different triads of cross ratios (discounting some symmetries, since there are 768 such triads in all) whose product is CR1234. Some of these relations among cross ratios on the same six points are:

(CR1236)(CR5634)(CR1654) = (CR6234)(CR1265)(CR1564)

(CR6234)(CR1254)(CR5264) = (CR1236)(CR1534)(CR1635)

(CR1634)(CR5236)(CR1256) = (CR1264)(CR6235)(CR6534)

(CR1235)(CR6534)(CR1564) = (CR5234)(CR1256)(CR1654)

(CR5234)(CR1264)(CR6254) = (CR1235)(CR1634)(CR1536)

(CR1534)(CR6235)(CR1265) = (CR1254)(CR5236)(CR5634).

These relations can be used to show that no more than five cross ratios can be measured independently on six points. (I would like to thank Bernard E. Rodden for these items of calculus.)Then parsimony does not rule out the use of cross ratio in a standard of measure, any more than parsimony rules out the use of the Cartesian coordinate system for similar numbers of points. The reader may be interested to know that the relations I have cited hold for the analogue of cross ratio for five and six points in the plane, where CR1234 is taken to be $(\Delta_{12\varnothing}\Delta_{34\varnothing})/(\Delta_{23\varnothing}\Delta_{41\varnothing})$, as used for the dependent measures of experiments 1 through 3A of the present article.

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The evaluation of risk communication effectiveness

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Risk communication deals with important and ambitious objectives. Main tasks are: advancing knowledge about risk issues, influencing risk-related behavior, and facilitating cooperative conflict resolution. However, are these goals actually achieved by the employed risk communication strategies? To answer this question, evaluation research is necessary, i.e., the assessment of the content, process and outcomes of respective interventions according to defined criteria. A look at current risk communication activities shows that, altogether, rather few evaluation projects have been conducted so far. Based on a review of selected empirical studies, methodological possibilities and difficulties are discussed. Clarifying the causation structure of risk communication obviously is the key problem, and valid evidence of a program's effectiveness is often restricted. Yet increased evaluation efforts which meet strict standards are indicated for several reasons: the purposes of risk communication are substantial and sometimes urgent; risk communication is difficult, controversial and usually expensive, requiring justification; and evaluation results are the best means to improve risk communication programs. It is recommended to incorporate evaluation measures into a risk communication program in advance and as an integral part of it.

Risk communication is a young, rapidly growing field of social-scientific risk research (see, e.g., Covello et al. 1986; Davies et al. 1986; Jungermann et al. 1991; Kasperson and Stallen 1990; O'Riordan 1983; Plough and Krimsky 1987). This interest has been induced by large-scale, public conflicts about the impacts of new large-scale technologies (such as nuclear power or genetic engineering) as well as by the limited success of safety campaigns, prevention of hazards (both natural and technological ones) and health protection programs. Com-

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munication about the meaning and the consequences of individual, societal and ecological risks has become a crucial issue. It includes aspects of risk perception, risk evaluation and risk management.

Programs on risk communication deal with important and ambitious *objectives*. Main tasks (as proposed by, e.g., Covello et al. 1986; Jungermann et al. 1988; Kasperson 1986; Keeney and von Winterfeldt 1986) include: identifying controversial aspects of perceived risks, presenting and explaining risk information, educating about risk assessment, increasing the public's risk awareness, changing attitudes toward risk, influencing risk behavior of individuals and encouraging protective action, developing information strategies for emergency cases and improving disaster warnings, evolving procedures of cooperative decision making in risk management and joint problem solving for conflicts.

Obviously 'risk communication' is a rather general term and there is no established and coherent definition of the field – but in principle the tasks listed above are related to three primary goals:

- Advancing/changing knowledge and attitudes (A),
- Modifying risk-relevant behavior (B),
- Facilitating cooperative conflict resolution (C).

Accordingly, actual risk communication activities differ considerably in substantive issues, audiences/actors, information channels and communicative situations (see table 1 for an overview).

Any process of risk communication involves a variety of parties. There are six types of '*actors*' (Rohrmann 1991): risk-exposed people (employees, residents, consumers, patients, etc.); the general public; industry/manufacturers/companies (being one but not the only 'risk-producer'); administrative/regulatory authorities; media; and scientists. The most frequent path of risk information is from official agencies to exposed or concerned people as the main 'target audience' (Fessendenraden et al. 1987; Fischhoff 1987; Renn 1991). However, if interactive types of risk communication are included, any actor is both communicator and audience. This view is in line with a definition of 'risk communication as an interactive process of exchange of information and opinion among individuals, groups, and institutions' (National Research Council 1990). Individual and corporate actors in risk communication use manifold communicative means and channels, depending on the 'arena' of interaction (see examples in table 1). Key

Table 1

Components of the risk communication process.

Basic types of risk communication	
– Advancing/changing knowledge and attitudes	(A)
– Modifying risk-related behavior	(B)
– Facilitating cooperative conflict resolution	(C)
Target audiences and actors	
– Risk-exposed residents, employees, consumers, patients etc.	
– The general public	
– Industry/manufacturers/companies	
– Journalists/media	
– Administrative/regulatory authorities	
– Science	
Communication channels	
– Television	
– Broadcasting	
– Newspapers and journals	
– Brochures etc., distributed by institutions	
– Product information, machine-operating instructions, etc.	
– Public information services, 'hot lines' etc.	
– Personal presentations (at public meetings)	
Communicative situations, 'arenas'	
– Information campaigns by agencies or companies	
– Safety training courses, e.g., at work	
– Public hearings, conferences etc.	
– Judicial proceedings	
– Counseling contacts	
– Articles/investigative reporting in mass media	
– Protest activities/events	
– Private situations	

issues are health protection, safety improvement and – particularly in the case of controversial technologies – risk acceptance.

The question of effectiveness

Risk communication is a permanent activity by governmental, industrial and private organizations. Many different strategies have been developed, and a considerable number of public programs have been conducted. (For overviews and examples – although restricted to the USA – see the appendices in Covello et al. 1989a,b; or Fisher et

al. 1991; see also the bibliographies of Hammond and Victor 1988; and Rohrmann et al. 1990). However, do risk communication activities actually achieve their goals, i.e., are the strategies employed effective with respect to the objectives of the actor? To answer this question, evaluative research is an essential requirement.

'*Evaluation*' means the assessment of the content, process and effects (consequences, outcomes, impacts) of an intervention (measure, strategy, program) and their appraisal according to defined criteria (goals, objectives, values) (Patton 1986; Rossi and Freeman 1985; Wottawa and Thierau 1990). Systematic empirical investigations are required in order to prove the effectiveness of risk communication – simple experience is not sufficient.

Reasons for empirical evaluation studies

Evaluation efforts are necessary for several substantial and methodological reasons:

- If the goals of risk communication are important (and sometimes urgent), it is important to prove its effectiveness.
- Evaluation results can demonstrate not only if and when but also *why* a program works (or not) and thus facilitate the improvement of current or future risk communication efforts. (This applies for both completed and on-going activities.)
- Intuitive assessments of the program's effectiveness can easily fail because of wrong cause–effect attributions.
- The effectiveness of risk communication might be context-dependent; therefore it is useful to analyze which strategies work in which situations.
- Evaluation provides an empirical basis for a decision between alternative risk communication programs.
- As risk communication is difficult and usually rather expensive (in terms of financial costs, personnel and time), evaluation can help to justify the efforts.

Of course evaluation may also be (mis)used for instrumental purposes such as gaining legitimation, 'canalizing' trouble with the public, avoiding or postponing the 'real' risk management, 'white-wash' intentions (Kasperson and Palmlund 1989; Vlek and Keren 1992), and even serve as a 'war tool' in controversies about risk communication – but even so the question of effectiveness remains valid.

Table 2

Criteria for risk communication effectiveness.

Evaluation aspects	Info source		
Substantial criteria			
Content evaluation			
– Substantive correctness		E	
– Completeness of the information	A	E	
– Comprehensibility of the message		E	R
– Congruence between message and info need/request			R
– Belief/trust in the information			R
– Attention-calling ability		E	R
– Ethical considerations		E	
Process evaluation			
– Difficulties/failures in running the program	A		
– Inclusion of relevant actors/societal groups	A	E	
– Feedback possibilities		E	R
– Facilitation of communication		E	R
– Constructive interaction between involved parties		E	R
– Relevant target groups reached		E	R
Outcome evaluation			
– Degree of information dissemination		E	R
– Reception of the information provided			R
– Increased/improved knowledge			R
– Advanced problem awareness and involvement			R
– Consistency/homogeneity of responses to messages			R
– Acceptance of the message			R
– Confidence of the information source			R
– Change of beliefs/attitudes			R
– Improved risk-controlling behavior			R
– Amount of participatory activities		E	R
– Number of responses to the RC agency	A		
– Reduction of accident/illness/mortality rates	A	E	
– Conflict resolution	A		R
Organizational criteria			
– Financial efficiency (material costs, personnel)	A		
– Time requirements	A		
– Training needs for the personnel involved	A	E	
– Difficulty of implementing the program	A		
– Flexibility and adaptability (content, procedure)		E	

Note:
'A' stands for RC agency (or author of the RC program),
'E' for risk expert or RC expert (independent researchers),
'R' for information receiver or participant of the RC program.

Measuring risk communication effectiveness

Criteria for risk communication programs

In principle, there are two perspectives for evaluating risk communication: Firstly, the *objectives* of risk communication activities may be evaluated according to, e.g., societal or ethical or political aspects. Secondly, once the objectives of a risk communication program have been stated it can be assessed – based on a methodological perspective – whether they have been *achieved* or not. This second approach will be discussed next.

The key problem is to identify and to measure criteria for the *effectiveness* of risk communication. Effectiveness can be defined as the degree to which an initial (unsatisfactory) situation is changed toward an intended state, as defined by the program goals. This overall criterion has to be explicated by characteristics of the content, the process and the outcomes of the risk communication program employed. A list of respective effectiveness criteria is presented in table 2. It is based on an analysis of risk communication objectives and a review of existent evaluation studies. Recommendations for improving risk communication (e.g., Covello et al. 1989b; National Research Council 1990) have also been used as information sources.

The first set of criteria refers to the question whether the *content* of the message and its presentation is valid for the communication goals. Correctness, completeness and comprehensibility of the information are crucial, but it is equally important to consider whether the message stimulates attention, meets the actual information needs of the receiver and is rated as personally relevant, is perceived as believable and not frightening.

The second set of criteria is related to the *process* of conducting risk communication programs. Of course, the success of the program employed depends on whether the relevant actors/parties have been identified, reached and motivated to exchange information. In this context, feedback possibilities are important, particularly if the focus is on two-way communication.

The third set in table 2 deals with the actual *outcomes* of risk communication. Even if content and process of the program (which might be considered as 'instrumental' goals) meet their respective objectives, achieving the intended effects cannot be guaranteed. Suffi-

cient information dissemination and reception by the defined target group are preconditions for being effective. But a good program should also actually improve the receiver's comprehension, knowledge, problem awareness and involvement, and eventually change beliefs, attitudes and behaviors – thus these are the crucial criteria. In this context it is relevant whether the recipients develop sufficient confidence in the information source and accept the message. Increasing trust in risk-communicating authorities (see, e.g. Renn and Levine 1991) might be considered as a meta-goal.

Many criteria are difficult to operationalize and to measure in empirical studies. Both, reliability and validity deserve careful consideration. For example, as shown by many social-psychological studies, the link between knowledge or attitudes and behavior is usually weak (see McGuire, 1985, for a review). Thus cognitive variables seem insufficient to use as criteria for behavioral changes. Also, measures of behavior intentions and self-reported behavior may lack validity for the actual behavior. Clearly it depends on the type of the risk communication activity – advancing knowledge (A), modifying risk behavior (B), facilitating conflict resolution (C) – which criteria are appropriate in order to measure effectiveness. Most content criteria are mainly relevant for types (A) and (B) while process criteria have particular importance for type (C). Which outcome criteria refer to (A) or (B) or (C) is self-evident. Often, 'hard' outcome data are not available; in that case content or process criteria can be useful substitutes.

In addition to substantial (goal-related) criteria, *organizational* and procedural aspects are to be included in the evaluation of risk communication programs (see the last group of criteria in table 2). Such 'secondary' criteria include practicability (flexibility, adaptability, implementability) and particularly costs (in terms of money, personnel, and time) of the program. Of course economic characteristics are vital for any agency, and data on costs are needed for cost/benefit or cost/effectiveness analyses (see Wortman, 1983, for an overview).

Data types and sources for effectiveness measures

Most of the criteria discussed above are defined as individual responses; others can be measured on an *aggregated* level only. The most general data are accident figures, health statistics, or mortality

rates of the population. Another type of criteria is achievement of conflict resolution; here the unit of observation would be the problem case. The same is true for organizational criteria.

A further issue is the *source* of evaluative data. If cognitive, motivational or behavioral effects of a program are to be measured, those who receive risk information or participate in a cooperative risk communication activity are the obvious respondents; see column 'R' in table 2. Additionally these respondents should belong to the population exposed to the risk (or the group causing the risk) dealt with in the specific risk communication program. Otherwise findings may lack external validity as it is difficult to simulate real risk situations. However, not all criteria can be appropriately judged by such respondents. Other variables require either the expert judgment of those who created, implemented and conducted the program, and/or assessments by independent experts are needed, such as experts on the substantive issues treated in the risk information, administrative experts, and experts on communication processes (see columns 'A' and 'E' in table 2). In consequence, many evaluation studies will need two or three sets of respondents in order to get a comprehensive and valid set of evaluation data.

Determinants of risk communication effects

The basic evaluation model is quite simple: risk communication is seen as an intervention causing certain effects which are measured by effectiveness criteria. But obviously there are more factors influencing the process and the results of risk communication efforts. Any program takes place in a complex social context, and the observed outcomes result from an interaction of program features (content, source, channel), personal characteristics of the information receiver (perceived risk exposure, involvement, information needs, abilities, prior knowledge, attitudes, feelings, etc.) and external influences, such as other related events or organizational constraints.

For both the conceptualization and the interpretation of an evaluation study it is instructive to develop a *process model* for risk communication. Key elements are risk perception, attitudes toward risk issues, and risk-related behavior (for useful theoretical approaches see, e.g., Slovic et al. 1980; Wilde 1982; Borcherding et al. 1986; Lindell 1986; Renn, in press; Zimmermann 1987; Earle et al. 1990).

Also, social-psychological processes of knowledge acquisition, persuasion, and attitude change are to be considered (see, e.g., Ajzen and Fishbein 1980; McGuire 1985; Petty and Caccioppo 1982; Petty et al. 1990).

Fig. 1 shows a basic (rather simplified) framework; the initial structure indicates that hazards lead to risk perception and risk behavior, and that these responses are moderated by personal characteristics of the exposed people and by the social environment (see the left part of fig. 1). Since reactions to risks and risk information constitute a longitudinal process, this structure appears twice, in the 'ante' and in the 'post' condition, i.e., before and after the risk communication activity. Furthermore, it is assumed that the content of the risk communication program (the message) actually changes risk perception and risk behavior (of course this is the core assumption of the model). Attitudes concerning risk communication (i.e., trust in the communicator, attitude toward the suggested risk-reducing behaviors, belief in the efficacy of measures, etc.) are significant additional determinants, being influenced by the risk communication context and the social environment.

While changes in risk perception and risk behavior are intended, and the central matter of interest, person characteristics and attributes of the social environment may also change their status (partly induced by the intervention, partly by other 'external' events). Therefore, in an analysis of risk communication effects, only some of the respective variables can be treated as stable across time (cf. solid versus dotted lines in fig. 1).

Given the many influences on risk communication effectiveness, systematic measurements of response variables *and* co-determinants – possibly before and after the intervention – and a careful description of the content, the course and the circumstances of the risk communication program are necessary in order to get a full understanding of its effects.

Designing evaluation studies

Substantial requirements

First of all, a clear statement of the goals of a risk communication program is necessary. From these goals evaluation criteria are to be

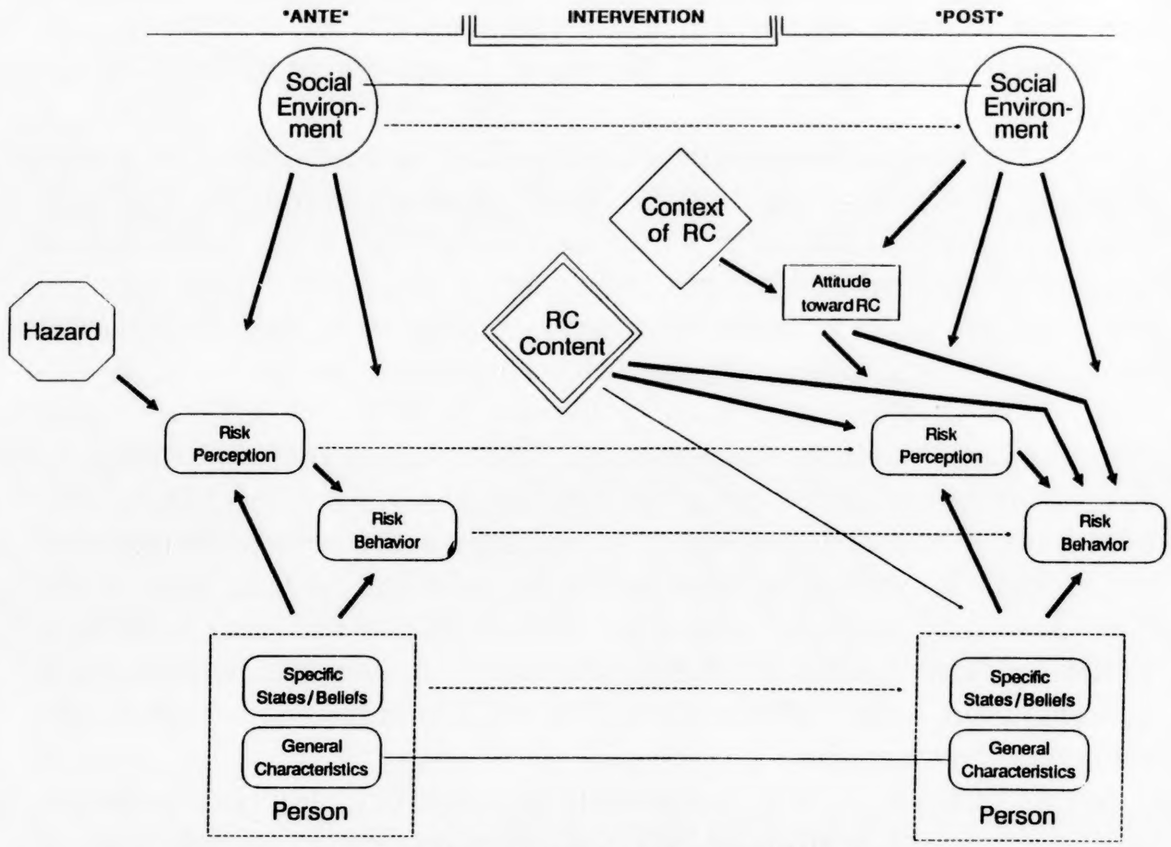

Fig. 1. Risk communication: A process framework.

derived. The criteria must be defined in advance (prior to the execution of the program). Also, change-related variables should be measurable before and after the intervention in order to allow for quantitative comparisons. Furthermore, the evaluator must decide which perspective is most useful for the evaluation task: *content*-orientation (i.e., input/message evaluation), *outcome*-orientation (i.e., summative/impact evaluation), or *process*-orientation (i.e., formative evaluation).

In each case advanced research designs are required. Evaluative data can be gathered in an analytical assessment done by experts or in an empirical investigation in which pertinent participants are surveyed (cf. table 3). Program evaluation is a highly developed area of social-scientific research, offering a wide range of approaches (Cook et al. 1985; Patton 1986; Rossi and Freeman 1985). As outlined above, a process-oriented theoretical framework and a careful consideration of relevant actors are indispensable as well.

Table 3

Types of risk evaluation studies.

Focus	content-oriented (substantive quality)	
	outcome-oriented (summative effectiveness)	
	process-oriented (formative/developmental view)	
Data collection	analytical	
	empirical	cross-sectional
		longitudinal
Information source	internal	sender/author/agency
		receiver
	external	risk expert
		communication expert
Reference for comparisons	normative program goals	
	previous situation	
	alternative RC strategies	

The critical 'valuation' of the obtained findings is the crucial step in an evaluation study. The stated (normative) program goal is the principal, but not the only reference for assessing evaluation results. Comparisons with the previous situation or (if feasible) comparative effectiveness measures of alternative interventions (other risk communication strategies or possibly other risk prevention approaches) are important as well.

Methodological considerations

Designing an evaluation study should conform to the general principles of social-scientific research. In the following, some main points will be discussed briefly (for a thorough discussion of evaluation research designs see, e.g., Cook and Reichardt 1992; Herman et al. 1988; Rossi and Freeman 1985; Wottawa and Thierau 1990). The focus is on prospective, quantitative evaluation.

Target population(s). All groups involved in the risk communication process (participants, acting institutions etc.; cf. tables 1 and 2) should be included because the sampling approach will determine the scope and external validity (i.e., generalizability) of evaluation findings.

Table 4

Designing data collection in RC evaluation studies

	Relevant phases of the risk communication process				
	'ANTE'	INTERVENTION	'POST'-1	'POST'-2	'POST'-3
Specific investigation patterns for groups of respondents					
(A)		X	!		
(B)	!	X	!		
(C)	!	X	!	!	!
(D)		X		!	
(E)		X			!
(F)		! ! !			
(G)	!	! ! !	!	!	!
(H)		0	!		
(I)	!	0	!		

Note: ! = Data collection; 0 = Respondent *not* exposed to the risk communication program; X = Treatment but no investigation.

Longitudinal design and timing of data collection. The most simple investigation would be a 'post-facto' study; see pattern (A) in table 4. However, such an approach (although frequently adopted) is insufficient, as it usually does not enable valid conclusions. In principle a longitudinal pretest–posttest design is necessary (cf. pattern (B), in which a 'treatment group' is surveyed before and after the intervention). With respect to short-term versus long-term effects, further points in time are needed (e.g., pattern (C)). Especially in process evaluations test points 'in between', i.e., a longitudinal design for the intervention phase, will be indispensable (cf. pattern (F) or (G) which is extended into both the 'ante' and the 'post' phase).

The timing of the data collection phases can be crucial to distinguish between 'true' effects and biases such as overestimation or underestimation of effectiveness because of time-dependent experiences (for example, if the post-intervention data collection is done too early, slowly 'growing' outcomes of risk communication might not be detected). Furthermore, effects may change over time.

Control groups. Groups not exposed to the 'treatment' (if feasible) might be measured before and after the intervention phase or in the 'post'-situation only (patterns (H) and (I)). Using such groups together with treatment groups, e.g., a combination of patterns '(B) plus (I)'

can clarify whether changes in risk behavior occur regardless of the risk communication program. Sometimes, groups which are surveyed in one phase only can be useful (see patterns (D) and (E); for this purpose, pattern (A), although inappropriate by itself, is useful as well). Investigation patterns such as '(B) plus (A)' or '(C) plus (A) and (D) and (E)', enable to control for reactivity effects. As the necessary randomization of subjects to conditions or 'placebo-groups' is often impossible, quasi-experimental research designs (cf. Cook and Campbell 1979) are helpful to reduce threats to the internal validity of evaluation studies.

Reliability and validity. If – as usual – no established measures of the selected evaluation criteria (cf. fig. 1) are available, critical checks of measurement quality are needed (this task often is underestimated).

The methodological deliberations discussed so far have great influence on whether causal relationships can be established between the intervention and the effects that occur in view of many moderating/confounding variables (cf. table 3). Results are meaningless if it is not possible to disentangle outcomes of the program and of concurring extraneous events. Too simple approaches (or what Patton, 1986, calls 'quick and dirty' evaluation) are not appropriate in the rather complex case of evaluating risk communication effects. The advantages of analytical evaluations based on (quasi-)experimental research designs are obvious. However, descriptive case studies (see, for example, Keck 1984; Krimsky and Plough 1988; Pocchiari et al. and 1986; Sharlin 1986; Wiedemann et al. 1990) are beneficial as well. 'Quantitative' and 'qualitative' approaches provide different kinds of evidence and are often complementary.

Empirical examples

Available research

A variety of evaluations of risk communication activities has been conducted – mainly related to health issues or environmental problems. The risk sources include medical hazards (e.g., smoking, cancer, unhealthy diet, AIDS), technological hazards to which employees or

residents are exposed (e.g., nuclear energy, chemical plants/processes/products, air traffic), natural hazards (e.g., earthquakes, floods, radon radiation), and so forth.

There is much more research – although not (yet) *labeled* as 'risk communication' – in related areas, dealing with issues such as occupa-

Table 5

Evaluation research on RC effects: Some examples.

	Study				
	(1) Weinstein et al. (Rutgers) 1989	(2) Hirose (WCU, Tokyo) 1990	(3) Cummings et al. (CIS) 1987	(4) Smith et al. (Univ. + OPPE) 1987	(5) McGrath et al. (NHLBI) 1986
RC type	(A)	(A)	(A) (B)	(A) (B)	(A) (B)
Risk issue	Asbestos, Radon gas	Earth-quakes	Diet and cancer	Radon gas	High blood pressure
RC means	(Risk scales)	Info from mass media	TV info, Brochures	Brochures (2×2 types)	Multiple organiz. activities
Effect crit.	Comphrehens., Info eval., Behavioral intentions	R-awareness Emergency preparedness (reported)	Info use, Knowledge, Behavior (reported)	Knowledge, Info demand, R-reducing behavior	Disease awareness, Medical behavior
Study design	CS 7 experim. groups Survey Mail. quest.	CS Postfacto survey Media analysis	CS Postfacto Survery Mail. quest.	LT (t = 2) Panel 1EG/1CG Quasi-exp. group comp.	LT Postfacto Census data Surveys
Sample N	1948	1...	1106	EG = 2231/2087 CG = 250/182	(several 1000)
Target group	R-exposed residents	Exposed residents	Public and R-exposed	R-exposed residents	R-exposed people
Eval.	Internal	External	Internal	External	Internal

Abbreviations: (A) = advancing/changing knowledge/attitudes, (B) = modifying risk behavior, (C) = facilitating conflict resolution. R = risk, CS = cross-sectional, LT = longitudinal study, EG = experimental group, CG = control group. CIS = Cancer Information Service, Buffalo, NY (USA); Duke Univ. = Duke University at Durham, NC (USA); FZJ = Forschungszentrum Jülich (D); NHLBI = Nat. Heart, Lung and Blood Inst., Bethesda/VA (USA); OPPE = Office for

tional safety programs (e.g., hearing loss precaution), health education programs (e.g., dental care), accident prevention programs (e.g., seat belt campaigns).

Although several authors discuss the necessity of evaluation research (Arkin 1988; Covello et al. 1989a,b; Kasperson and Palmlund

(6) Ehlers (Univ. M.) 1987	(7) Earle et al. (WISOR) 1990	(8) Viscusi and O'Connor (Duke Univ.) 1987	(9) Hendrickx (TRC. Gron.) 1991	(10) Wiedemann et al. (FZJ) 1990
(A) (C)	(B)	(B)	(B)	(C)
Surgery	Pesticides, Water cont., Sex. diseases	Hazardous chemicals at work	Car driving	Waste management technologies
Verbal messages, Discussion	Verbal messages	Hazard warning labels	Brochures (frequency/scenario)	Public participation
Knowledge, Surgery decisions	Behavior intentions	Perception, Working behavior	Driving in blind curves	Informed consent, cooperative solution
CS Postfacto Survey Mail. quest.	CS 3 experim. groups Quasi-exp. group comp.	CS 4 experim. groups Quest.	CS Field exp. 4 info cond. Quest	CS 4 case studies Telephone interviews
108	139	335	64	$\Sigma = 40$
Patients	Students and spec. groups	Chemical workers	Pivate car drivers	Involved soc. groups
External	Internal	Internal	Internal	External

Policy, Planning and Evaluation of the EPA (Environmental Protection Agency, USA); Rutgers = Rutgers State University, NJ (USA); TRC Gron. = Traffic Research Center, University of Groningen; Univ. M = Universität München (D); WCU = Womens Christian University; WISOR = Western Institute for Social and Organizational Research, Bellingham, WA (USA).

1989; Kasperson and Rohrmann 1988; Leiss and Krewski 1990; Sorensen and Mileti 1987), reviews of existing risk communication programs show that altogether rather few explicit evaluation projects have been completed. For the majority of risk communication activities there is no empirical evaluation at all.

The following overview and discussion of empirical investigations focuses on the evaluation of risk communication efforts. The vast literature on risk cognition and risk behavior in, e.g., workplaces or traffic will not be discussed; for reviews see, e.g., Evans and Schwing 1985; Gstalter 1988; Hoyos 1987; McCormick and Ilgen 1985.

In order to go beyond abstract considerations, a – quite selective – set of examples shall be used. In table 5, 10 studies on risk communication effects are summarized: Cummings et al. (1987); Earle et al. (1990); Ehlers (1987); Hendrickx (1991); Hirose (1990); McGrath et al. (1987); Smith et al. (1987) (cf. Smith et al. 1990; Oppe 1988); Viscusi and O'Connor (1987) (cf. also Viscusi et al. 1987); Weinstein et al. (1989); Wiedemann et al. (1990). These studies cannot represent the whole range of the respective research but illustrate typical evaluation issues and approaches. The main characteristics will be briefly described (see table 5 for details):

Conducting institution: Most studies have been conducted either by universities or by research institutes related to governmental/administrative authorities. This might be biased, as non-academic research – e.g., the many activities of companies – is often unpublished.

Communicated risk issue: Environmental hazards, health risks and safety topics are predominant.

Risk communication means: Information campaigns, usually verbal messages presented in brochures or via mass media, are most frequent. Personal communication occurs, for example, in the case of medical decisions (see example (6) in table 5). Interactive discussion is the principal means in public committees on risk issues (cf. example (10)).

Effectiveness criteria: In most cases cognitive effects are investigated; e.g., changes in knowledge, beliefs and attitudes. Behavioral reactions are studied as well, but often the indicators are *reported* behavior or behavioral intentions rather than actually observed behavior.

Study design: Cross-sectional studies are the usual method of investigation. There are two types: surveys (based on personal or telephone

or mailed interviews) in which responses to risk communication activities are investigated in a 'post-facto' design; and (quasi-)experimental approaches in which sets of respondents receive different risk information in order to compare communication means and messages with respect to knowledge, attitude or behavior changes.

Longitudinal studies are still rare (study (4) is an example of a before–after design based on a panel). The explicit use of control groups is rather infrequent as well (but see (4) and (9) which included samples of people not exposed to the risk communication under study).

Most evaluation studies use specific problem-oriented samples and deal with individual responses while outcomes on the aggregate level (as in (5) or (10)) are less often investigated.

Target audience: All the studies dealt with those who received (or should have received) the risk information, but these are not always those who are actually exposed to the risks described in the risk communication program. This is particularly true if sampling is based on students or 'ad-hoc-available' respondents.

Evaluator: Many evaluations are 'internal'; i.e. conducted by the respective institution. Fortunately, the number of 'external' evaluation studies seems to be growing.

Discussion

Although the – slowly increasing – body of evaluation studies is very helpful, not all investigations meet strict standards. Some methodological *shortcomings* are:

- insufficient designs (e.g., no 'ante' data (prior to the intervention), no control groups),
- weak effectiveness criteria (e.g., based only on intended but not 'real' behavior),
- hypothetical situations and/or respondents not personally exposed to the risk,
- no stringent analysis of the causes of success or failure of risk communication strategies.

Furthermore, some types of risk communication such as emergency warnings or conflict resolution approaches (see e.g., Sorensen and Mileti 1987; Wiedemann and Femers 1990) were insufficiently consid-

ered in comprehensive evaluation studies. This also applies to the use of DSS software and expert systems within risk communication problems (see, e.g., Fiskel and Covello 1987; Otway and Haastrup 1987).

Due to such limitations, the valid empirical evidence of a program's effectiveness is often restricted. The same might be true for various risk communication 'manuals' or 'cardinal rules' (examples are Covello and Allen 1988; or Hance et al. 1989) in which quite strict recommendations are presented. (For a critique see Earle and Cvetkovich 1988; Otway and Wynne 1989.) In conclusion, more – and possibly improved – empirical evaluation research is needed in order to establish a solid body of valid findings which have general implications.

Difficulties, necessities, suggestions

Constraints and pitfalls

Obviously, evaluation is not an easy business. Most likely, the researcher will face considerable difficulties and constraints both in organizational and methodological terms. Typical problems are: limited funds, lack of institutional support, undefined or vague program goals, lack of baseline data, inappropriate sampling, measurement problems with criteria variables and judgmental biases, variability of effects over time, unclear validity of findings, and sometimes ethical questions.

To overcome threats to evaluation validity and interpretative pitfalls it is crucial to clarify the *causation structure* of the risk communication process. The primary aim is to assess whether implementation and conduct of the program actually lead to the intended results. However, the observed positive or negative outcomes of risk communication activities can also be caused by *'external' influences* (e.g., knowledge gained from other sources, changes in the hazard, situational circumstances) rather than by the program. Furthermore, the intervention may have not only desired results but also *unintended effects* (e.g., confusion, fear or reduced self-confidence of laymen, wrong (risk-increasing) behavioral consequences, distrust in the communicator).

The basic measurement problems with effectiveness criteria (cf. table 2) have already been discussed. Additionally, judgmental *ex-*

pectancy biases are to be considered for both the subjects and the researchers. Research on the 'social psychology of the experiment' (see, e.g., Adair 1973; Aronson et al. 1985; Rosenthal and Rosnow 1969) has identified a number of confounding effects:

- Subjects exposed to new conditions or possibilities (introduced as improvements) tend to react positively due to the mere existence of the program, independently of its particular function (so-called 'Hawthorne' effect). Processes of social perception (like selective information search or 'inertia' with existing personal hypotheses about 'reality') can increase such tendencies.
- Experimenters have particular expectations about the outcomes of their treatment, and they may – unintentionally – convey their hypotheses to their subjects ('Rosenthal' effect). In such a case the expectations would induce 'self-fulfilling prophecies'. This becomes very relevant if the author of the risk communication is his/her own evaluator.

These biases may cause a spurious (pseudo)effectiveness of the analyzed risk communication program.

Finally, although designs with repeated measurements are very useful, the problem of 'reactivity' (the fact that being a subject – and knowing about it – per se has effects on responses) becomes more serious the more often the respondents are asked to participate in the evaluation study. Careful planning of controls (e.g., 'placebo' groups) in the research design as well as data collection methods being as 'unobtrusive' as possible (Webb et al. 1981) will help to reduce the problems with expectancy biases and reactivity.

Context dependency of studies

Risk communication programs are realized in a specific setting which is determined by the characteristics of the target audience, the information source and sender, institutional preconditions, the societal context of the communication process, and so forth (Krimsky and Plough 1988; Leiss and Krewksy 1990; Renn 1991). This implies that a particular risk communication strategy might not work (or perhaps only works) for certain kinds of risk, certain institutions and communicators, certain types of audiences, certain political circumstances, etc. Therefore the implementation and the course of the program as well

as 'background' factors (e.g., administrative conditions, concurring events, media reports) should be carefully monitored. This requires cooperation with the organization conducting the risk communication, but, nonetheless, the evaluator must remain independent.

Concluding suggestions

The issues discussed above affect both the 'internal' and 'external' validity of findings (Cook and Campbell 1979). In consequence, the whole psychological and social context of risk communication should be taken into account in an evaluation approach.

Thus, rigorous state-of-the-art methods are required, as provided by the well-developed fields of social-scientific program evaluation or psychological approaches to intervention control. In principle 'quick-shot' research will not help, because it does not allow for unambiguous answers; hence findings can easily be discredited. Actually misleading knowledge might be worse than missing knowledge (Kasperson and Rohrmann 1988).

An important recommendation is to consider evaluation efforts in advance and to integrate them into the risk communication design – by that the evaluability of a program can be considerably increased, and unfavorable interactions between running the program and evaluating it can be avoided.

Finally, how about the utilization of findings? There is evidence that not enough attention is paid to evaluation results. Since ignored evaluation studies are useless, the thorough explanation and *dissemination* of the findings (as complex and contradictory as they may be) is very important. Furthermore, the *applicability* of research outcomes to future risk communication programs within relevant institutional settings should be explicated by the evaluation researcher.

Improving the effectiveness of information campaigns, behavior modification and cooperative strategies for risk management is the final purpose of evaluation efforts – thus results should be useful *and* usable for that aim.

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Book reviews

K. Landwehr (ed.), *Ecological Perception Research, Visual Communication, and Aesthetics*. Springer-Verlag, Berlin, 1990. 141 pp. ISBN 3-540-52200-X.

This book is the outcome of a meeting of eight students of perception and visual communication, which took place in Bonn in 1987. It contains 12 papers: an introduction, a section on pictures, plans, drawings, and displays (seven papers), a section on ecological aesthetics (three papers), and an epilogue.

According to the editor of the volume, visual communication and aesthetics have not yet been studied from an ecological point of view. The papers presented here comprise a first attempt to define the scope and direction of questions to be raised, the tools available, and critical items to be kept in mind. Most of the contributors agree that Gibson's ecological approach to visual perception provides a useful starting point for theorizing and empirical research in the area of visual communication and aesthetics.

In the introduction Landwehr sums up Gibson's approach to picture perception. For someone who is not familiar with the direct–indirect perception debate, this exposition is too fragmentary and obscure to understand what the debate really is about. The interested reader, therefore, has to look things up in other books, for example Gardner (1985), Gibson (1979, 1980), Hagen (1986), Sedgwick (1980), all of which gave much better expositions of the Gibsonian view of perception.

The second paper (Costall), too, sums up Gibson's view of picture perception. Gibson took picture perception as a special case. It is Costall's aim to identify the various reasons why Gibson took that stand. Costall concludes that one of the very significant contributions of Gibson is that he sets pictures aside as a special case. In his view, it is this contribution of Gibson rather than his attempt at a theory of pictures which is most important to our understanding of picture perception.

In the third paper, Espe presents 11 theses on pictures, most of which are trivial ('pictures differ from reality', 'the artist has at his disposal considerable degrees of freedom for the production of pictures', 'pictures cannot completely be translated into texts', etc.)

Deregowski's paper (paper 4) focuses on the distinction between pictures with and without direct cues for depth. Deregowski elaborated on this

interesting distinction before at great length (Deregowski 1989), and it also figures in his recent book (Parker and Deregowski 1990). The discussion of the two kinds of pictorial representation in the present volume adds nothing to the presentation in BBS, and one therefore can do without it.

Kennedy et al. (paper 5) describe an experiment on the use of pictures to communicate motion, and consider how different shapes can be taken to be the same object represented in various projection systems. Their aim is to produce evidence against the idea that observers have to infer depicted motion, which can be suggested and represented in pictures directly in many ways. They report an experiment in which subjects had to decide which picture (always a circle with a trail of arcs) shows which motion (slow, fast, accelerating, and decelerating). However, taking a glance at the pictures makes it clear that they do not elicit an impression of motion whatsoever! So the experiment tells us nothing about motion suggested by pictures, it is about motion suggested by verbal instruction (a demand characteristic).

According to Landwehr (paper 6) pictures tend to obscur the complexities of information by only showing its final integration. By decomposing that information, the relative importance of its separable aspects can be ascertained. In his view, surface texture is the most important kind of information needed to assess the impact of an architect's proposals. Unfortunately, the author does not report data concerning the effectiveness of the proposed procedure to decompose optical stimulus information.

Papers 7 (Lempp) and 8 (Krampen) are hardly informative. Lempp presents a series of pictures displaying the development of a computer working table. Krampen sums up obviously beneficial as well as undesirable aspects of pictures.

Krampen opens the section on aesthetics (paper 9) with a presentation of an ecological semiotics with special regard to surface layouts. In fact, this is the third time in this volume that Gibson's ecological approach is summed up.

Höge (paper 10) proposes a new definition of a picture: a picture is an ecologically restricted area, where action is restricted or even prohibited. In short: pictures are affordance-free. This is, of course, a reformulation of the old notion of aesthetic distance. Whether such a notion has any heuristic value remains to be seen (see Freedberg, 1989, for an excellent review of the power of images).

In the last paper of the aesthetics section, Landwehr reports an empirical investigation of the perceived beauty of a semi-natural landscape seen from a car driver's perspective. The main finding of his explorative study is that the aesthetic appreciation of a specific kind of landscape (areas used for industry, settlement, or farming, or woods) is a function of context, more specifically he finds contrast-effects: when industry comes into view after woodland,

there is a marked decrease in perceived beauty, and a marked increase if the sequence is reversed. This kind of contrast-effect has been reported in the literature many times before.

In the epilogue, Landwehr summarizes the topics discussed in this volume by formulating four claims: there is no all-embracing definition of a picture, pictures do not exist in a social vacuum, research till now failed to look at the possible uses of pictures, and an aesthetic of pictures is not to be had independently from an aesthetic of the basic surface layout which affords our living.

This volume is one of a series named *Recent Research in Psychology* which, according to the publisher, will make original papers, reports of conferences of exceptional interest, and other documents of high quality and broad interest timely available for wide distribution. However, as the reader already will have noticed, the volume under consideration does not answer any of these positive descriptions. I therefore cannot recommand this volume neither to the professional nor to the general reader.

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W.J. Friedman, *About Time*. MIT Press, Cambridge, MA, 1990.

This is a book on psychological time, written by the author of several outstanding research papers on the mental representation of time, in particular viewed from the perspective of developmental psychology. After an introductory chapter in which different types of temporal experiences are outlined, the seven subsequent chapters each highlight a different aspect of psychological time, covering perception, memory, cogntion, orientation, (ontogenetic) development, and variations in time awareness like those associated with different cultural background.

The book offers a lot of informative data and claims on how time is processed by humans in a diversity of life experiences. It does so in a remarkably lucid and narrative way, largely avoiding the use of technical jargon and quantitative formulations unfamiliar to a generally interested reader. The latter is even stimulated in didactically elegant ways to actively read what Friedman is reporting to him or her. Thus, for example, he asks in the beginning of chapter 3 (on mnemonic aspects of time) which of two previously used words ('beach' vs. 'theorizing') in that chapter was most recently encountered by the reader, and whether the Chernobyl disaster preceded the Three Miles Island or the reverse. Indeed, such a rhetoric strategy activates the reader to carefully anticipate on the outcomes of empirical studies that will follow. The transparency and vividness of style with which the author simplifies the conceptual difficulties associated with psychological aspects of time almost inevitably implies a shadow side. The latter is observed in the abundant use of methaphors which not seldom are introduced as clever maskers of lack of knowledge in this area of research. A beautiful and, in some sense self-referential, example is found at the end of chapter 4 ('Mental Models of a Temporal World') where the first sentence of the concluding remarks states; 'Having shaken the blackbox in various ways, we find that no single metaphor suffices for describing its contents' (p. 64).

The author has been successful in reviewing an important and representative number of facts and insights on psychological time. It certainly is not completely covering all behavioral domains where time is a crucial variable. For example, the role of time in performance situations like timing behavior in playing music is hardly touched upon. Such omissions are not to be blamed for, however, when an author – in this case Friedman – is explicit in telling the reader what kind of biases or preferences (s)he may expect.

Finally, I find it difficult to tell which readership may optimally benefit from reading the book. Probably, it is the generally interested reader with at least some academic background, or a scientist in a rather different area of

time research like physical or biological time, who may want to know how time is studied by psychologists.

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